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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From March 1, 1877, *to December* 20, 1877.

VOL. XXVI.

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CONTENTS.

VOL. XXVI.

No. 179.—*March* 1, 1877.

Page
List of Candidates for Election into the Society 1

Note on the Electrolytic Conduction of some Organic Bodies. By J. H. Gladstone, Ph.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, and Alfred Tribe, F.C.S., Lecturer on Chemistry in Dulwich College 2

On the Protrusion of Protoplasmic Filaments from the Glandular Hairs of the Common Teasel (*Dipsacus sylvestris*). By Francis Darwin, M.B. .. 4

On the Magnifying-power of the Half-prism as a means of obtaining great Dispersion, and on the General Theory of the "Half-prism Spectroscope." By W. H. M. Christie, M.A., Fellow of Trinity College, Cambridge 8

March 8, 1877.

On Magneto-electric Induction in Liquids and Gases.—Part I. Production of Induced Currents in Electrolytes. By J. A. Fleming, B.Sc. 40

On the Structure and Development of Vascular Dentine. By Charles S. Tomes, M.A. 43

Note on the Early Stages of Development of the Nerves in the Chick. By A. Milnes Marshall, B.A., B.Sc., of St. John's College, Cambridge 47

Notes on Physical Geology. By the Rev. Samuel Haughton, M.D. Dublin, D.C.L. Oxon., F.R.S., Professor of Geology in the University of Dublin.. 51

March 15, 1877.

On the Tides of the Arctic Seas.—Part VII. Tides of Port Kennedy, in Bellot Strait. (Final Discussion.) By the Rev. Samuel Haughton, M.D. Dublin, D.C.L. Oxon., F.R.S., Fellow of Trinity College, Dublin 63

Page

Studies in the Chinoline Series.—I. Transformation of Leucoline into Aniline. By Prof. James Dewar, M.A. 65

On the Density of Solid Mercury. By Prof. J. W. Mallet, University of Virginia 71

The Automatic Action of the Sphincter Ani. By W. R. Gowers, M.D., Assistant Physician to University College Hospital 77

Description of the Process of Verifying Thermometers at the Kew Observatory. By Francis Galton, F.R.S. 84

March 22, 1877.

On Stratified Discharges.—IV. Stratified and Unstratified Forms of the Jar-Discharge. By William Spottiswoode, M.A., Treas. R.S.............. 90

On Friction between Surfaces moving at Low Speeds. By Fleeming Jenkin, F.R.SS. L. & E., Professor of Engineering in the University of Edinburgh, and J. A. Ewing.. 93

Magnetic Observations at Kerguelen. By the Rev. S. J. Perry, S.J., F.R.S. 95

On the Variations of the Daily Range of the Magnetic Declination as recorded at the Kew Observatory. By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at Owens College, Manchester 102

List of Presents 121

No. 180.—*April* 12, 1877.

On certain Molecular Changes which occur in Iron and Steel during the separate acts of Heating and Cooling. By Professor Norris, M.D., Queen's College, Birmingham.. 127

On the Rapidity of Growth and Variability of some MADREPORARIA on an Atlantic Cable, with remarks upon the rate of accumulation of Foraminiferal Deposits. By Prof. P. Martin Duncan, F.R.S., Pres. Geol. Soc. .. 133

On Attraction and Repulsion of Bubbles by Heat. By Walter Noel Hartley, F R.S.E., F.C.S., King's College, London 137

On the Constant Vibration of Minute Bubbles. By Walter Noel Hartley, F.R.S.E., F.C.S., King's College, London 150

April 19, 1877.

On Putrescent Organic Matter in Potable Water. By Gustav Bischof.... 152

On a Cause for the Appearance of Bright Lines in the Spectra of Irresolvable Star-Clusters. By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope 156

On some Figures exhibiting the Motion of Vibrating Bodies, and on a New Method for determining the Speed of Machines. By Herbert M'Leod, F.C.S., Professor of Experimental Science, and George Sydenham Clarke, Lieut. R.E., Instructor in Geometrical Drawing in the Royal Indian Engineering College, Cooper's Hill 157

April 26, 1877.

Page

On the Nature and Origin of the Beds of Chert in the Upper Carboniferous Limestones of Ireland. By Prof. Edward Hull, M.A., F.R.S., Director of the Geological Survey of Ireland. With Chemical Notes, by E. T. Hardman, F.C.S., of the Geological Survey of Ireland 163

Researches on Emeralds and Beryls.—Part II. On some of the Processes employed in the Analysis of Emeralds and Beryls. By Greville Williams, F.R.S. 165

On Repulsion resulting from Radiation.—Preliminary Note on the Otheoscope. By William Crookes, F.R.S. &c. 176

On the Inferences to be drawn from the Appearance of Bright Lines in the Spectra of Irresolvable Nebulæ. By William Huggins, D.C.L., LL.D., F.R.S. 179

List of Presents 181

On the Temperature of the Human Body in Health. By Sydney Ringer, M.D., Professor of Therapeutics at University College, London, and the late Andrew P. Stuart, M.R.C.S. 186

No. 181.—*May* 3, 1877.

Further Observations on the Modification of the Excitability of Motor Nerves produced by Injury. By George J. Romanes, M.A., F.L.S. 211

On the Temperature-correction and Induction-coefficients of Magnets. By G. M. Whipple, B.Sc., Superintendent of Kew Observatory 218

Distribution of the Radicals of Electrolytes upon an Insulated Metallic Conductor. By Alfred Tribe, Lecturer on Chemistry in Dulwich College 222

May 17, 1877.

On Hyperjacobian Surfaces and Curves. By William Spottiswoode, M.A., Treas. R.S. 226

On the Length of the Spark from a Voltaic Battery in different Gases at ordinary Atmospheric Pressures. By Warren De la Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. 227

Further Researches on the Deportment and Vital Resistance of Putrefactive and Infective Organisms, from a Physical point of view. By John Tyndall, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution 228

May 31, 1877.

The Physical Properties of Homologues and Isomers. By Frederick D. Brown, B.Sc. 238

On the Amplitude of Sound-waves. By Lord Rayleigh, M.A., F.R.S. 248

Page

On the alleged Correspondence of the Rainfall at Madras with the Sun-spot Period, and on the True Criterion of Periodicity in a series of Variable Quantities. By General Strachey, R.E., C.S.I., F.R.S. 249

List of Presents 262

On the Calculation of the Trajectories of Shot. By W. D. Niven, M.A., F.R.A.S. 268

No. 182.—*June* 7, 1877.

Election of Fellows 288

June 14, 1877.

On the Minute Structure and Relationships of the Lymphatics of the Mammalian Skin, and on the Ultimate Distribution of Nerves to the Epidermis and Subepidermic Lymphatics. By George Hoggan, M.B., and Frances Elizabeth Hoggan, M.D. 289

Refractive Indices of Glass. By J. Hopkinson, D.Sc., M.A. 290

Electrostatic Capacity of Glass. By J. Hopkinson, D.Sc., M.A. 298

On the Difference of Potential produced by the Contact of different Substances. By R. B. Clifton, F.R.S., Professor of Experimental Philosophy in the University of Oxford 299

The Physiology of Sugar in relation to the Blood. By F. W. Pavy, M.D., F.R.S. 314

Correction of Statement in the "Note on the Electromotive Properties of Muscle," read December 14, 1876. In a Letter to Professor Huxley, by Dr. Burdon Sanderson, F.R.S. 322

Photographic Image of Stratified Discharges. A Letter to Prof. Stokes, Sec. R.S., by W. Spottiswoode, M.A., Treas. R.S. 323

On the Length of the Spark between two Spherical Surfaces of the Chloride-of-Silver Battery. By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. 324

June 21, 1877.

On the Normal Paraffins.—Part II. By C. Schorlemmer, F.R.S., Professor of Organic Chemistry in Owens College, Manchester 325

The Relationships of the Nerve-cells of the Cortex to the Lymphatic System of the Brain. By Bevan Lewis, F.R.M.S., Assistant Medical Officer at the West Riding Asylum. (Plates 1 & 2.) 326

On the Thickness of Soap Films. By A. W. Reinold, M.A., Professor of Physics in the Royal Naval College, Greenwich, and A. W. Rücker, M.A., Professor of Physics in the Yorkshire College of Science, Leeds .. 334

Page

The Physiology of Sugar in relation to the Blood.—No. 2. By F. W. Pavy, M.D., F.R.S. 346

Note on Dr. Burdon Sanderson's latest Views of Ferments and Germs. By J. Tyndall, D.C.L., LL.D., F.R.S. 353

Experimental Demonstration in respect to the Origin of Windings of Rivers in Alluvial Plains, and to the Mode of Flow of Water round Bends of Pipes. By Professor James Thomson, LL.D., D.Sc., F.R.S. 356

An Attempt to form Double Salts of Nitrate of Silver and other Nitrates. By W. J. Russell, Ph.D., F.R.S., and Nevil Story Maskelyne, F.R.S. 357

On certain Definite Integrals. By W. H. L. Russell, F.R.S 359

List of Presents 363

On the Hindoo Division of the Octave, with some Additions to the Theory of Systems of the Higher Orders. By R. H. M. Bosanquet, Fellow of St. John's College, Oxford 372

No. 183.—*June* 21, 1877 (*continued*).

On some hitherto Undescribed Optical Properties of Doubly Refracting Crystals.—Preliminary Notice. By H. C. Sorby, F.R.S., President of the Royal Microscopical and of the Mineralogical Societies 384

On the Foci of Lines seen through a Crystalline Plate. By G. G. Stokes, M.A., Sec. R.S. 386

Notes on Physical Geology.—No. III. On the probable Age of the Continent of Asia and Europe; and on the Absolute Measure of Geological Time. By Rev. S. Haughton. M.D., D.C.L., F.R.S., Professor of Geology in the University of Dublin 401

On the Increase in Resistance to the Passage of an Electric Current produced on Wires by Stretching. By Herbert Tomlinson, B.A., Demonstrator of Natural Philosophy, King's College, London 401

No. 184.—*November* 15, 1877.

The Bakerian Lecture.—On the Organization of the Fossil Plants of the Coal-measures.—Part IX. On the latest Researches into the Organization of the Fossil Plants of the British Coal-measures, especially of the *Calamites* and *Lepidodendra*. By W. C. Williamson, F.R.S., Professor of Natural History, Owens College, Manchester 411

November 22, 1877.

Remarks on the Attributes of the Germinal Particles of *Bacteria*, in Reply to Prof. Tyndall. By J. Burdon Sanderson, M.D., LL.D., F.R.S. 418

November 30, 1877.

Page
Anniversary Meeting:—
  Report of Auditors 426
  List of Fellows deceased since last Anniversary 427
  ———— elected 427
  Address of the President 427
  Presentation of the Medals 446
  Election of Council and Officers 451
  Number of Fellows 451
  Financial Statement 452
  Trust Funds 454–456

Account of Grants from the Donation Fund in 1876–77 457

Account of the appropriation of the sum of £1000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science 457

Account of appropriations from the Government Fund of £4000 made by the Lords of the Committee of Council on Education, on the recommendation of the Council of the Royal Society 458

Report of the Kew Committee 461

List of Presents 479

December 6, 1877.

On the Tides at Malta. By Sir G. B. Airy, K.C.B., Astronomer Royal .. 485

Observations on Hermetically-sealed Flasks opened on the Alps. In a Letter to Professor Huxley, Sec. R.S. By Professor Tyndall, LL.D., F.R.S. .. 487

Researches on the Effect of Light upon *Bacteria* and other Organisms. By Arthur Downes, M.D., S.Sc. Cert. Cantab., and Thos. P. Blunt, M.A. (Oxon.), F.C.S. 488

Points of Resemblance between the Suprarenal Bodies of the Horse and Dog, and certain occasional Structures in the Ovary. By Charles Creighton, M.B., Demonstrator of Anatomy, Cambridge University 500

December 13, 1877.

On Electrostriction. By Edmund J. Mills, D.Sc., F.R.S., "Young" Professor of Technical Chemistry in Anderson's College, Glasgow 504

The Examination of Air. By R. Angus Smith, Ph.D., F.R.S. 512

On a Cause for the Appearance of Bright Lines in the Spectra of Irresolvable Star-Clusters. By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope 517

Experimental Researches on the Electric Discharge with the Chloride-of-Silver Battery.—Part I. By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. 519

December 20, 1877.

Page
Notes on Supersaturated Saline Solutions. By Charles Tomlinson, F.R.S. 523

Notes on Physical Geology.—No. III. On a new Method of finding Limits to the Duration of certain Geological Periods. By the Rev. Samuel Haughton, M.D. Dubl., D.C.L. Oxon., F.R.S., Professor of Geology in the University of Dublin 534

On certain Movements of Radiometers. By G. G. Stokes, M.A., Sec. R.S. 546

List of Presents 555

Obituary Notices of Fellows deceased:—

Dr. Sibson i
J. B. A. L. L. Élie de Beaumont iv

Index 563

ILLUSTRATIONS.

Plates 1 and 2 illustrating Mr. Bevan Lewis's Paper on the Relationships of the Nerve-cells of the Cortex to the Lymphatic System of the Brain.

ERRATUM IN VOL. XXV.

Page 508, equation (21.), in the denominator of the integral *for* cos θ *read* cos tan θ.

ERRATA IN VOL. XXVI.

Page 53, line 4 from bottom, *for* foot *read* mile.
„ 357, line 4, *delete* the hyphen in material-like.
„ „ footnote, line 5 from bottom, *for* Helvingrove *read* Kelvingrove.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

March 1, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read, as follows:—

John Anderson, M.D., F.R.S.E., F.L.S.
John Attfield, Ph.D., F.C.S.
Francis Maitland Balfour, F.L.S.
Charles Barry, Pres. R.I.B.A.
Prof. Robert Bentley, F.L.S.
Walter Lawry Buller, Sc.D., F.L.S.
George Chesney, Lieut.-Col. R.E.
William Chimmo, Capt. R.N.
Prof. James Henry Cotterill, M.A.
Rev. William Henry Dallinger.
Herbert Davies, M.D.
Prof. James Dewar, M.A.
Sir Thomas Fairbairn, Bart.
Sir Joseph Fayrer, M.D., K.C.S.I.
Rev. Norman Macleod Ferrers, M.A.
George Fleming, M.R.C.V.S.
Thomas Richard Fraser, M.D., F.R.S.E.
William Galloway.
Henry Haversham Godwin-Austen, Major.
Rev. William Greenwell, M.A., F.S.A.
George Griffith, M.A.
Brian Haughton Hodgson, F.L.S.
John Hopkinson, M.A., D.Sc.
John Hughlings Jackson, M.D., F.R.C.S.
John W. Judd, F.G.S.
William Carmichael M'Intosh, M.D., F.L.S.
Robert M'Lachlan, F.L.S.
Richard Henry Major, Sec. R.G.S.
Prof. John William Mallet, Ph.D.
Henry B. Medlicott, M.A., F.G.S.
Henry Nottidge Moseley, M.A.
William Donald Napier, M.R.C.S.
Prof. Henry Alleyne Nicholson, M.D., Ph.D., D.Sc.
Richard Norris, M.D.

Thomas Bevill Peacock, M.D., F.R.C.S.
William Overend Priestley, M.D., F.R.C.P.
Charles Bland Radcliffe, M.D., F.R.C.P.
Prof. Osborne Reynolds, M.A., C.E.
Samuel Roberts, M.A.
William Roberts, B.A., M.D.
George F. Rodwell, F.R.A.S., F.C.S.
George John Romanes, M.A.
Sir Sidney Smith Saunders, C.M.G.
Edward A. Schäfer, M.R.C.S.
Michael Scott, M.Inst.C.E.
Samuel Sharp, F.G.S., F.S.A.
John Spiller, F.C.S.
Hermann Sprengel, Ph.D.
George James Symons, Sec. M.S.
Prof. James Thomson, M.A., LL.D., D.Sc.
Charles S. Tomes, M.A.
Edwin T. Truman, M.R.C.S.
Prof. William Turner, M.B., M.R.C.S.
Thomas Lord Walsingham.
Benjamin Williamson, M.A.
Prof. John Wilson, F.R.S.E., F.G.S.
Baron Henry de Worms, F.R.A.S.

The following Papers were read:—

I. "Note on the Electrolytic Conduction of some Organic Bodies." By J. H. Gladstone, Ph.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, and Alfred Tribe, F.C.S., Lecturer on Chemistry in Dulwich College. Received January 27, 1877.

During our early researches[*] on the copper-zinc couple it naturally occurred to us that we were employing a special means of electrolysis acting at insensible distances; but the first organic substances which we succeeded in decomposing by means of this agent were such as are usually considered non-electrolytic—for instance, iodide of ethyl; and we obtained the remarkable result that when some of these were mixed with alcohol they were much more readily decomposed, although pure alcohol itself is not attacked by the couple of dissimilar metals. From time to time we experimented with external batteries of 10 or 50 cells of Grove, in order to obtain some direct evidence of the electrolyzability of these compounds in the ordinary way, but with only negative results. On the 26th of April, 1875, however, we made a series of experiments, employing 100 Grove's cells, and obtained results which we did not pursue further at the time, but which we think interesting, especially in connexion with the experiments on other but similar bodies which Dr. Bleekrode has lately communicated to the Royal Society, and a short notice of which appears in the last Number of its 'Proceedings.'

We used for the experiments a glass tube, about 5 millims. diameter, closed at one end, into which were fused two platinum wires, about

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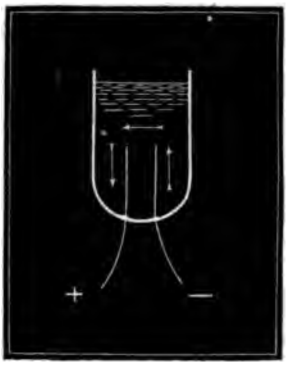
[*] Proc. Roy. Soc. 1872, p. 218. Journ. Chem. Soc. 1873, pp. 445, 678, 961; 1874, pp. 208, 406, 410, 615; 1875, p. 508.

1 millim. apart. The liquids were placed in this tube, and the wires connected with the terminal wires of the battery, an ordinary astatic galvanometer being placed in the circuit. The results obtained are as follows:—

Ethyl Iodide.—Nil.

Alcohol.—On making contact the galvanometer-needle showed a deflection of 20°, and a slight commotion of the liquid was observable. The alcohol gradually warmed up until it boiled, the deflection of the needle increasing the whole time. Gas was apparently given off in minute quantity from the negative electrode; but it was difficult to be certain of this.

Equal volumes of Alcohol and Ethyl Iodide.—There was a deflection of 30° on making contact, gradually rising. Great commotion ensued, the liquid rapidly circulating round the poles, as in the annexed diagram, accompanied by a browning of the liquid. Not certain about the evolution of gas—apparently a few minute bubbles from the positive pole. The liquid boiled in about 4 minutes, the deflection being then 60°.



Ethyl Bromide.—Nil.

Equal volumes of Alcohol and Ethyl Bromide.—Violent commotion on making contact, the galvanometer being deflected to the stops. The liquid quickly boiled. Gas apparently given off from the negative electrode.

Chloroform.—Nil.

Equal volumes of Alcohol and Chloroform.—Violent commotion. Deflection to stops. The mixture very quickly boiled.

Ethyl Acetate, Propylene Bromide, Amyl and Isobutyl Iodides gave negative results. When mixed with equal volumes of alcohol they behave similarly to the mixtures referred to above.

Our results, preliminary as we considered them to be, show that the iodides of ethyl, isobutyl, and amyl, the bromides of ethyl and propylene, the acetate of ethyl, and chloroform are practically non-conductors to a battery-power of 100 cells Grove, and that alcohol is to some extent traversed by the current. They show also that when these liquid non-conductors are mixed with the feeble conductor alcohol, the conductivity of the mixture is greater than that of alcohol alone, which offers at least a partial clue to the readiness with which such mixtures are decomposed by the copper-zinc couple.

The very considerable development of heat in these liquids, which conduct the electric current with great difficulty, is a circumstance worthy of notice. In these cases it is evident that it does not result from any chemical change, because the decomposition, if any thing at all, is utterly insignificant in amount.

II. "On the Protrusion of Protoplasmic Filaments from the Glandular Hairs of the Common Teasel (*Dipsacus sylvestris*)." By Francis Darwin, M.B. Communicated by Charles Darwin, F.R.S. Received January 30, 1877.

(Abstract.)

The protoplasmic structures described in the following communication are connected with the glandular hairs or trichomes found on both surfaces of the leaf of the common teasel (*Dipsacus sylvestris*). The trichomes are of two kinds, differing in a marked manner in shape. The form of gland from which alone the protoplasmic filaments issue is shown in the diagram. The gland consists of a multicellular pear-shaped head, supported on a cylindrical unicellular stalk which rests on a projecting epidermic cell. The whole structure projects about $\frac{1}{10}$ of a millimetre ($\frac{1}{250}$ inch) above the surface of the leaf.

The filaments issue from inside the gland-cells, reaching the surrounding medium by passing through the external cell-wall of the gland. The point where protrusion takes place is on the summit of the gland, and usually at the point of junction of several radiating cells at the centre of its dome-like surface. The act of protrusion is rapidly effected; a previously naked gland may be seen to send forth a minute thread of trembling protoplasm, projecting from its summit freely into the surrounding water. The filament grows by clearly visible increments, and may ultimately attain the length of nearly one millimetre. The filaments appear to pass through the substance of the external cell-wall of the glands, as no apertures to allow of their passage have been observed.

Under normal circumstances the filament presents the appearance of a delicate and elongated thread slightly clubbed at its free end, and animated by the perpetual tremble of Brownian movement. The distal end of the filament is often attached to the gland, thus forming a loop. Extremely delicate filaments of great length are often seen entangled in elaborate and complex knots, or several filaments may be seen issuing from a single gland.

The substance of which the filaments are composed is gelatinous, transparent, highly refracting, and devoid of granules. It is in a great measure soluble in alcohol, is stained by tincture of alkanet, and not blackened by osmic acid, and is coloured yellow by iodine. These reactions, when combined with results of various physiological tests, show that the filaments contain resinous matter in some way suspended in protoplasm.

The most remarkable point in the behaviour of the filaments is their power of violently contracting. The act of contraction commences by the filament becoming shorter and thicker at a number of nearly equidistant points, situated close together near the free end of the filament. The curious beading thus produced spreads rapidly down the filament, which ultimately runs violently together into a ball seated on the top of the gland. In other cases contraction takes place without any previous appearance of beading.

Filaments frequently break loose but retain their vitality, and are still capable of contraction although separated from their parent glands; and this observation is of importance, as proving that the movements of the filaments are not governed by forces residing within the glands, but that the filaments are composed of an essentially contractile substance.

The contraction of the filaments is produced by the following causes:—

Dilute acids (from 1 to ⅕ per cent.)—Sulphuric, hydrochloric, acetic, citric, and osmic acids.

Dilute alkaline solutions (¼ to ½ per cent.)—Carbonates of ammonia, sodium, potassium.

Solutions of gold-chloride ½ per cent., silver nitrate ¼ per cent., sulphate of quinine ⅒ per cent., citrate of strychnia (about) ¾ per cent., camphor ⅒ per cent., the poison of the cobra (about) ¼ per cent., iodine ¼ per cent.

Glycerine.

Methylated spirits.

Vapour of chloroform.

Heat. The temperatures at which the filaments contract are rather variable, but are all below 57° C.

Electricity. The induced current causes contraction.

Mechanical stimulation.—The filaments contract when pressure is made on the cover-glass.

The evidence derived from the experiments, of which the results are here briefly summarized, appears to be strongly in favour of the view that the filaments contain true living protoplasm, and that the sudden movement above described is a true act of contraction; for if the latter hypothesis is rejected, the only remaining view is that the filaments are so constituted as to be capable of undergoing coagulation, by which contractility is mechanically simulated. But it seems inconceivable that reagents of widely different natures, *such as dilute* solutions of acetic acid, of camphor, and of gold-chloride, should produce identical chemical effects. Osmic acid is well known to kill protoplasmic structures without making them contract. This characteristic reaction holds good with the filaments of the teasel when treated with sufficiently powerful solutions of osmic acid (*e.g.* 1 per cent.). When killed in an extended position, they cannot be made to contract with strong acetic acid. This observation is of importance in another way; for it proves that the violent movements caused by dilute acetic acid are of a "vital," and not simply of a chemical nature. Moreover the general character of the reagents and other causes (such as heat, &c.) by which contraction is produced is quite consistent with the belief that the filaments are protoplasmic in nature.

An important series of phenomena are produced by the following fluids:—dilute solutions ($\frac{1}{2}$ or $\frac{1}{4}$ per cent.) of carbonates of ammonia, potassium, and sodium, and infusion of raw meat. If a filament under the microscope is treated with a drop of $\frac{1}{4}$ per cent. solution of carbonate of ammonia, the following changes occur. The filament contracts, but almost instantly recovers itself, and is once more protruded. The filament, however, does not regain its original form or general appearance: instead of consisting of thin elongated ropes of a highly refracting substance, it is converted into balloon-like or sausage-shaped masses of very transparent, lowly refracting matter. These transparent masses are remarkable for the spontaneous changes of form and other quasi-amœboid movements which occur among them.

Dilute infusions of meat cause a similar effect, astonishing quantities of transparent matter being produced.

It has been shown that the filaments are protoplasmic bodies, containing a large quantity of resinous matter. The question next arises, with what process in plant-physiology is the protrusion of filaments homologous?

The leaf-glands of the teasel are similar in general structure to many glandular hairs which produce resinous and slimy secretions, and, like these glands, they contain bright drops of secreted resin lying in the centres of the gland-cells; they also resemble many glandular hairs in being often capped with accumulations of secreted matter. Now these accumulations stain red with alkanet, yellow with iodine, and are largely soluble in alcohol; that is to say, they consist of substances which have the same reactions as the filaments. There is, in fact, no doubt that the caps of resinous matter on the teasel-glands are produced by the accumulation of dead filaments. According to this view, the act of protrusion is essentially a process of secretion: the resin issues from the gland-cells, mingled with a certain amount of true protoplasm; and it is only from the death of the living or protoplasmic part of the filaments that the resinous accumulation results. This view of the act of protrusion corresponds with the theory of secretion held by some physiologists, viz. that secreted matter is produced by the dissolution or death of protoplasm—that, for instance, the oil in a fat-cell is the result of the disintegration of a plastid or individualized mass of protoplasm formed in the cell by endogenous cell-formation.

The protrusion of protoplasmic filaments from the glands of the teasel appears to bear an obscure relationship to the phenomena of "aggregation" in *Drosera* and several other plants. In both processes we have homogeneous highly refracting protoplasmic masses, which undergo amœboid movements, and are in some unknown way connected with the absorption of nitrogenous matter. In *Drosera* the protoplasmic masses remain within certain cells; in *Dipsacus* they are protruded through the cell-wall.

When we begin to inquire as to the function of the filaments, the answer seems at first to be sufficiently plain; but this is very far from being the case. The connate leaves of the teasel form cup-like cavities, which become full of rain and dew and in which many drowned insects accumulate. The glands at the base of the leaves are thus exposed to a highly nitrogenous fluid. And since such fluids are known to produce a remarkable effect on the filaments exposed to them, it seems probable that the filaments are in some way connected with the assimilation of food material. It seems probable that, either with or without the assistance of their filaments, the glands do absorb some nitrogenous matter; for changes of their cell-contents occasionally occur which can only thus be interpreted. But on account of the rarity and uncertainty of these aggregation changes *within the glands*, but little weight must be allowed to the phenomena as a proof of the absorbing capacity of the glands. Some other points in the structure of the plant render it almost certain that the connate leaves are specially adapted to serve some useful purpose. Kerner is probably right in believing that the "cups" of the teasel are of use to the plant in keeping off nectar-stealing ants and other wingless insects; but unless this is their only function, it seems probable that the connate leaves have been to a certain extent adapted for the capture of insects whose decaying remains are absorbed by the plant. The leaves are smooth and steeply inclined, and form a pair of treacherous slides leading down to a pool of water.

It is worthy of note that the leaves of the first year's growth, which do not form cups, are not smooth, but bristle with long sharp hairs; moreover in *Dipsacus pilosus* the leaves (of the second year's growth) are not sufficiently connate to form cups, and they also are rough with hairs. These facts seem to show that the smoothness of the second-year leaves in *D. sylvestris* is a specially acquired quality. Another special point of structure in *D. sylvestris* may be noted. The stems are everywhere armed with sharp prickles, except where they are covered by the water in the "cups;" and here they are quite smooth, so that no ladder of escape is afforded to the drowning victims. Even if we grant from the above considerations that the filaments protruded from the glands are in some way connected with the absorption of nitrogenous matter from the putrid fluid in the cups, we are far from understanding the whole of the problem; for precisely similar *filament-protruding* glands are found on the seedling leaves of *D. sylvestris* and on the second year's leaves of *D. pilosus*; and as no "cups" are formed in either of these cases, the filaments cannot be connected with absorption of the products of decay. The only view which suggests itself is that the filaments absorb ammonia from the dew and rain. Recent researches have shown that certain leaves have the power of absorbing an appreciable quantity of ammonia; and this fact lends some probability to the view above advanced.

To recapitulate. Protoplasmic filaments are protruded from the leaf-glands of the teasel; and the only theory which seems at all capable of connecting the observed facts is the following:—That the glands on the teasel were aboriginally (*i. e.* in the ancestors of the Dipsacaceæ) mere resin-excreting organs; that the protoplasm which comes forth was originally a necessary concomitant of the secreted matters, but that, from coming in contact with nitrogenous fluids, it became gradually adapted to retain its vitality and to take on itself an absorptive function; and that this power, originally developed in relation to the ammonia in rain and dew, was further developed in relation to the decaying fluid accumulating within the connate leaves of the plant.

III. "On the Magnifying-power of the Half-prism as a means of obtaining great Dispersion, and on the General Theory of the 'Half-prism Spectroscope.'" By W. H. M. CHRISTIE, M.A., Fellow of Trinity College, Cambridge. Communicated by Dr. HUGGINS. Received January 25, 1877.

On account of the oblique incidence of the rays on the isosceles prism and the consequent diminution of the aperture of the collimator, a "half-prism," formed by dividing an isosceles prism by a plane perpendicular to the base, has frequently been employed for the commencement of the train of prisms, and also for the end, though apparently without due consideration of the effect of the "half-prism" on the dispersion of the other prisms preceding in the train. This is a matter of some importance; for it will be found that when the angles of incidence and emergence are unequal (as in the half-prism), the angular separation between two pencils of parallel rays is increased or diminished according as the angle of emergence is greater or less than the angle of incidence. In consequence of this the angle between the pencils corresponding to any two lines in the spectrum, *e. g.* the two D lines, will be increased by passing through a half-prism (independently of the effect of ordinary dispersion) if the perpendicular face be turned towards the slit. At the same time the angle between the pencils coming from the two edges of the slit will, for any monochromatic light, also be increased in nearly the same ratio, and thus the lines in any spectrum become broader as they are further separated by virtue of this property, and the purity of the spectrum remains unaltered. In fact the half-prism is from this point of view equivalent to a cylindrical magnifier, a property which may prove most useful in the construction of the spectroscope, leading to the substitution of three or, at the most, four half-prisms for the long train of fifteen or twenty isosceles prisms now used. For this purpose direct-vision prisms will be most suitable; but I propose to begin by considering the simpler case of a half-prism of flint. The expression "Magnifying-power" may be conveniently extended to the property in question, which, as will be seen presently, is strictly analogous to that of a lens; for it will be found that the same law holds with combinations of prisms as with other optical combinations, viz. the magnifying-power is the ratio of the breadth of the incident to the breadth of the emergent pencil*.

In consequence of the property of the half-prism stated above, it becomes necessary to distinguish between separation accompanied by increased breadth of the lines and separation in terms of this breadth. If the sense of the term "dispersion" had not been fixed by usage, it might very well be applied to the latter effect, retaining "separation" for the other; but it will undoubtedly be more in conformity with received ideas to define "dispersion" as the angular length of the spectrum, whilst the term "purity" may be used for the ratio of this angular length to the angular breadth of the spectrum lines (expressed in terms of the angular width of the slit). In the case of an isosceles prism, for which the magnifying-power is 1, the dispersion and purity are equal.

I will therefore distinguish the two positions in which a half-prism can be placed with respect to the incident rays as "Magnifying" and "Diminishing," according as the perpendicular face is turned towards the slit or away from it. It will be found that the first position will be suitable in cases where wide separation of the lines is required, and the second where great purity in the spectrum is of importance; but before entering on this point and on the further comparison with the ordinary combinations of isosceles prisms, it is desirable to find a convenient expression for the dispersion in different cases, confining this term to the effect of the variation in the refractive index.

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* This law follows at once from the principle of conservation of energy, since the total energy of all the pencils which pass through the slit and fall (in a state of parallelism) on the prism is measured by the product of the breadth of the pencil into the angular width of the slit. If, therefore, by the refraction at any surface the breadth of the pencil is diminished, the angular breadth of the monochromatic image of the slit must be increased, the narrower pencils being spread over a larger angle, since the total energy remains constant. This is of course the ordinary law of brightness of optical images; a more formal proof from purely geometrical considerations is given in the text.

SIMPLE PRISM.

Let ϕ, ϕ' be the angles of incidence and refraction.

ψ, ψ' be the angles of emergence and refraction, ϕ and ψ being considered positive when they fall on the side of the normal away from the edge of the prism.

μ the refractive index.

α the refracting angle of the prism.

m the magnifying-power.

Δ the dispersion.

$\Pi=\frac{\Delta}{m}$ the purity.

D the deviation.

Then

$$\sin\phi=\mu\sin\phi', \qquad (1)$$
$$\sin\psi=\mu\sin\psi', \qquad (2)$$
$$\phi'+\psi'=\alpha \qquad (3)$$

and

$$D=\phi+\psi-\alpha.$$

1. *Magnifying-power.*

Let $\delta\phi$=angle between two incident pencils.

$\delta\psi$=angle between the two corresponding emergent pencils.

Then

$$\text{Magnifying-power } m=-\frac{\delta\psi}{\delta\phi}.$$

Now (1) and (2) give, μ being constant,

$$\cot\phi\,\delta\phi=\cot\phi'\delta\phi', \text{ and } \cot\psi\,\delta\psi=\cot\psi'\delta\psi';$$

and from (3)

$$\delta\phi'=-\delta\psi',$$

$$m=\frac{\delta\psi}{\delta\phi}=-\frac{\delta\phi'}{\delta\phi}\cdot\frac{\delta\psi}{\delta\phi'};$$

$$\therefore m=\frac{\cot\phi}{\cot\phi'}\times\frac{\cot\psi'}{\cot\psi}=\frac{\cos\phi}{\cos\phi'}\cdot\frac{\cos\psi'}{\cos\psi}=\frac{\text{Breadth of incident pencil}}{\text{Breadth of emergent pencil}},$$

as will be seen at once by drawing perpendiculars to the incident, refracted, and emergent pencils. Also, if

$$m'=\frac{\cot\phi'}{\cot\phi}, \qquad m''=\frac{\cot\psi'}{\cot\psi},$$

we have

$$m=\frac{m''}{m'}.$$

We may call m', m'' the magnifying-powers of the first and second surfaces respectively, whence,

$$\text{Magnifying-power of half-prism}=\frac{\text{Magnifying-power of second surface}}{\text{Magnifying-power of first surface}}.$$

2. *Dispersion.*

Taking μ as variable,

$$\delta\phi = \frac{\delta\mu}{\mu}\tan\phi + \frac{\cot\phi'}{\cot\phi}\,\delta\phi', \qquad \delta\psi = \frac{\delta\mu}{\mu}\tan\psi + \frac{\cot\psi'}{\cot\psi}\,.\,\delta\psi',$$

or

$$\delta\phi = \Delta' + m'\delta\phi', \qquad \delta\psi = \Delta'' + m''\delta\psi',$$

where

$$\Delta' = \frac{\delta\mu}{\mu}\tan\phi = \text{dispersion at first surface},$$

$$\Delta'' = \frac{\delta\mu}{\mu}\tan\psi = \text{dispersion at second surface};$$

also

$$\delta\phi' + \delta\psi' = 0, \text{ and } \delta\mathrm{D} = \delta\phi + \delta\psi.$$

Eliminating $\delta\phi'$ and $\delta\psi'$, we have

$$m''\delta\phi + m'\delta\psi = m''\Delta' + m'\Delta'',$$

with the further relations,

$$\left.\begin{aligned} \delta\phi - m'\delta\phi' &= \Delta', \\ \delta\psi - m''\delta\psi' &= \Delta''. \end{aligned}\right\}$$

3. *Purity.*

Putting $\Pi' = \frac{\Delta'}{m'} = \text{purity at first surface},$

$$\Pi'' = \frac{\Delta''}{m''} = \text{purity at second surface},$$

we have

$$\Pi' = \frac{\delta\mu}{\mu}\tan\phi', \text{ and } \Pi'' = \frac{\delta\mu}{\mu}\tan\psi';$$

$$\therefore\ m''\delta\phi + m'\delta\psi = m'm''\,(\Pi' + \Pi'').$$

Before proceeding further we must assume some relation between $\delta\phi$, $\delta\psi$, $\delta\phi'$, $\delta\psi'$ which will give a fourth equation; and here three cases may be considered.

i. $\delta\phi = 0$, or the collimator fixed relatively to the prism. This gives the dispersion and purity of the spectrum as seen by the eye, whatever be the form of spectroscope used.

ii. $\delta\phi' = -\delta\psi' = 0$, or the collimator and telescope movable relatively to the prism in such a manner that the pencils for different parts of the spectrum all pass through the prism in the same direction. This is the condition in the ordinary form of spectroscope with automatic adjustment to minimum deviation.

iii. $\delta\mathrm{D} = 0$, or the collimator and telescope relatively fixed, whilst the prism is turned so as to bring different parts of the spectrum into the field of view. This can only be done when the angles of incidence and emergence are unequal, as in the half-prism spectroscope.

Taking these three cases respectively, we have

i. $\delta\phi=0$; $\therefore \Delta=m\Delta'+\Delta''$, $\Pi=m'(\Pi'+\Pi'')$.

ii. $\delta\phi'=-\delta\psi'=0$; $\therefore \delta\phi=\Delta'=m'\Pi'$, $\delta\psi=\Delta''=m''\Pi''$, and $\delta D=\Delta'+\Delta''$ $=m'\Pi'+m''\Pi''$.

iii. $\delta D=0$; $\therefore \delta\phi=-\delta\psi=$ angle through which the prism is turned $=\theta$.

Then $\theta=\dfrac{m''\Delta'+m'\Delta''}{m''-m'}=\dfrac{m\Delta'+\Delta''}{m-1}=\dfrac{\Delta}{m-1}=\dfrac{m}{m-1}\Pi$.

In the second and third cases the scales of the dispersion and purity are variable, and are given by (i) for the part of the spectrum under examination on substituting the corresponding values of ϕ, ϕ', ψ, ψ' in m', m'', Δ', Δ''.

The three forms of simple prism which we have specially to consider are :—I. Half-prism magnifying; II. Half-prism diminishing; III. Isosceles prism.

I. *Half-prism magnifying.*

1. Magnifying-power.. $m_{,}=\dfrac{m''}{\mu}=\dfrac{\cos\psi'}{\cos\psi}$, since ϕ and $\phi'=0$ and $m'=\mu$.

2. Dispersion .... $\Delta_{,}=\Delta''=\dfrac{\delta\mu}{\mu}\tan\psi=m_{,}\delta\mu\tan\psi'$, since $\Delta=0$.

3. Purity............ $\Pi_{,}=\delta\mu\tan\psi'$.

II. *Half-prism diminishing.*

Keeping ϕ and ϕ' as the angles at the perpendicular face, which is now that of emergence, and taking $m_{,,}$, $\Delta_{,,}$, and $\Pi_{,,}$ as the values of the magnifying-power, dispersion, and purity in this case, we have :—

1. Magnifying-power $m_{,,}=\dfrac{1}{m_{,}}=\dfrac{\cos\psi}{\cos\psi'}$, m' and m'' being interchanged.

2. Dispersion ......$\Delta_{,,}=\dfrac{1}{m_{,}}\Delta''+\Delta'=\dfrac{\Delta_{,}}{m_{,}}=\dfrac{1}{m_{,}}\cdot\dfrac{\delta\mu}{\mu}\tan\psi=\delta\mu\tan\psi'$,

since $\Delta'=0$.

3. Purity .... $\Pi_{,,}=\dfrac{\dfrac{\Delta_{,}}{m_{,}}}{\dfrac{1}{m_{,}}}=\Delta_{,}=m_{,}\Pi_{,}=\dfrac{\delta\mu}{\mu}\tan\psi=m_{,}\delta\mu\tan\psi'$.

III. *Isosceles prism.*

1. Magnifying-power .. $m_{,,,}=1$, since $\phi=\psi$, $\phi'=\psi'$, and $m'=m''$.

2. Dispersion .... $\Delta_{,,,}=\Delta'+\Delta''=2\dfrac{\delta\mu}{\mu}\tan\psi=2m'\,\delta\mu\tan\psi'$.

3. Purity............ $\Pi_{,,,}=\Delta_{,,,}=2\Delta_{,}$.

These results might also have been obtained by considering the prism as made up of two half-prisms separated by a thin plate of air. The dispersion of the first half is $\frac{\Delta_{,}}{m_{,}}$, and this is magnified $m_{,}$ times by the second, which also adds its own dispersion $\Delta_{,}$, thus giving $\frac{\Delta_{,}}{m_{,}} . m_{,} + \Delta_{,} = 2\Delta_{,}$. This will be seen at once to be equivalent to the algebraic process employed.

The above results may be summed up as follows:—

1. The purity of a half-prism magnifying is equal to the dispersion of a half-prism diminishing, and is the product of the dispersive power of the glass and the tangent of the angle of the prism.

2. The dispersion of a half-prism magnifying is equal to the purity of a half-prism diminishing, and is m times the purity of a half-prism magnifying.

3. The purity and dispersion of an isosceles prism are equal, and are each twice the dispersion of a half-prism magnifying.

In future when the letters m, Δ, and Π are used without suffixes, they must be understood as denoting the magnifying-power, dispersion, and purity of a half-prism magnifying.

Confining our attention for the present to the ordinary form of a spectroscope in which $\delta\phi' = -\delta\psi' = 0$, the most important question to be considered is the irrationality of dispersion, which in fact fixes the limit to the refracting angle of the prism, unless the spectroscope is to be used only for one part of the spectrum.

The formula for the dispersion is chiefly useful as giving the irrationality, which is seen to increase with the dispersion, but in a more rapid ratio. In fact if we put Δ_A and Δ_H for the dispersions corresponding to the same small value of $\frac{\delta\mu}{\mu}$ for the Fraunhofer lines A and H, *i. e.* the values of the scale of the spectrum at those two points, we have

$$\log\frac{\Delta_H}{\Delta_A} = \log\tan\psi_H - \log\tan\psi_A = \frac{\sec^2\psi}{\tan\psi}(\psi_H - \psi_A) \text{ approximately}$$

$$= \frac{2(\psi_H - \psi_A)}{\sin 2\psi} = \frac{\delta\mu}{\mu} . \sec^2\psi.$$

In practice, however, it will be more convenient to express the ratio in terms of the magnifying-powers for A and H; thus

$$\frac{\Delta_H}{\Delta_A} = \frac{m_H}{m_A} . \frac{\delta\mu_H}{\delta\mu_A},$$

if the prism be adjusted to minimum deviation in each case.

In the following Table the angles of emergence, the dispersion, and the magnifying-power are given for a series of half-prisms of very dense flint, the refractive indices for A and H being $\mu_A = 1{\cdot}700$ and $\mu_H = 1{\cdot}770$. The refracting angle of the half-prism $= \psi'$. In this case

$$\Delta = \frac{1}{25} \tan \psi \text{ and } \frac{\Delta}{m} = \frac{1}{14} \tan \psi'.$$

Half-prism of Flint.

Dispersion and Magnifying-power.

ψ'.	ψ_H.	ψ_A.	Δ.	$m_H = \frac{\cos\psi'}{\cos\psi_H}$.	$m_A = \frac{\cos\psi'}{\cos\psi_A}$.
22°	41° 35′	39° 36′	1° 59′	1·24	1·20
24	46 5	43 47	2 18	1·32	1·27
26	50 50	48 7	2 43	1·42	1·35
28	56 7	52 52	3 15	1·57	1·45
30	62 15	58 13	4 2	1·86	1·64
32	69 44	64 17	5 27	2·45	1·95
34	81 40	71 48	9 52	5·72	2·66
34½	..	74 20	..	..	3·05
35	..	77 10	..	..	3·69
35½	..	80 45	..	..	5·12
36	..	87 43	..	..	20·20

Light reflected at emergence.

ψ'.	Polarized		Total.
	in plane of incidence.	in perpendicular plane.	
°	per cent.	per cent.	per cent.
30	H 28·5	0·0	14·3
	A 22·4	0·0	11·2
32	H 39·1	2·5	20·8
	A 28·9	0·5	14·7
34	H 67·1	27·7	47·4
	A 40·5	4·8	22·7
34½	A 45·9	8·3	27·1
35	A 52·2	13·3	32·8
35½	A 62·6	24·8	43·7
36	A 94·4	71·6	83·0

The above numbers express the percentage of light lost by reflexion, computed by Fresnel's formulæ for H and A respectively.

These Tables will enable us readily to compare prisms of different angles in the case of this particular quality of glass, which has been selected as giving large dispersion with moderate absorption. Let us take two examples for comparison with direct-vision prisms.

1. Isosceles prism of 60°.

$\Delta = 8^\circ\, 4'$. Loss of light $\left.\begin{array}{l}\text{for } H = 25 \text{ per cent.}\\ \text{for } A = 20 \text{ per cent.}\end{array}\right\}$

Irrationality of dispersion

$$2\delta\psi_A = 2m_A\, \delta\mu \tan\psi' = \cdot 135 \text{ or } 7^\circ\, 44'.$$
$$2\delta\psi_H = 2m_H\, \delta\mu \tan\psi' = \cdot 153 \text{ or } 8^\circ\, 46'.$$

2. Isosceles prism of 64°.

$2\Delta = 10^\circ\, 54'$. Loss of light $\left.\begin{array}{l}\text{for } H = 34 \text{ per cent.}\\ \text{for } A = 25 \text{ per cent.}\end{array}\right\}$

Irrationality of dispersion

$$2\delta\psi_A = 2m_A\, \delta\mu \tan\psi' = \cdot 174 \text{ or } 9^\circ\, 58'.$$
$$2\delta\psi_H = 2m_H\, \delta\mu \tan\psi' = \cdot 218 \text{ or } 12^\circ\, 30'.$$

In computing the loss of light after passing through several surfaces, the two polarized beams must of course be taken separately and afterwards combined.

The quantities given under Irrationality of Dispersion represent what would be the length of the spectrum if the scale were the same throughout as at A and H respectively. This is independent of irrationality in the refractive index of glass as referred to wave-lengths.

If we had taken an ordinary flint, the above numbers would be considerably modified. Thus taking $\mu_A = 1\cdot 6200$, and $\mu_H = 1\cdot 6666$, which correspond to a moderately dense flint, Fraunhofer's No. 30, specific gravity 3·7, we should have

For a prism of 60°,

$$\psi_A = 54^\circ\, 6', \quad \psi_H = 56^\circ\, 25', \text{ dispersion} = 4^\circ\, 38'.$$

COMPOUND PRISM.

Let $\left.\begin{array}{l}\beta \text{ be the refracting angle}\\ \mu' \text{ the refractive index}\end{array}\right\}\left\{\begin{array}{l}\text{of the prism of crown glass which is}\\ \text{cemented to the half-prism of flint,}\end{array}\right.$

χ, χ' the angles of emergence and refraction at the outer surface of the crown,

the other angles being denoted as before, though the relation between ψ and ψ' will now be

$$\mu' \sin\psi = \mu \sin\psi'.$$

Two cases here present themselves according as β is greater or less than ψ. The former gives the direct-vision prism, and the latter the compound prism commonly used in large spectroscopes.

In the first case we have $\psi + \chi' = \beta$, in the second $\psi - \chi' = \beta$; but this can be reduced to the first case by considering χ' as negative. In conformity with this we shall consider the angles of refraction as positive when they fall on the side of the normal towards the edge of the prism (whether flint or crown) in which the refraction takes place, and the angles of incidence or emergence as positive when they fall on the side from the edge.

Thus the two forms of compound prism may be considered together; but there is this important distinction, that the refraction at the outer surface of the crown corrects irrationality of dispersion in the direct-vision prism, since the angle of refraction is greater for the red rays, whilst in the ordinary compound prism the irrationality is greatly increased. At the same time the refraction at the outer surface of the crown slightly decreases the dispersion for the whole spectrum in the direct-vision prism, and slightly increases it in the other form; but this effect is produced in the latter case by spreading out the violet rays, and in the former by bringing them closer together, the dispersion for the red rays being about the same for the same angles of refraction in the two forms, as will be seen more clearly when an actual case is taken. Thus the apparent superiority of the ordinary compound prism turns out to be illusory, being gained by inequality in the scale; and this is aggravated by corresponding loss of light for the violet rays, as the deviation at *both* refractions is greater than for the red rays. On the other hand, both the scale and the loss of light are equalized throughout the spectrum in a properly constructed direct-vision prism, which is therefore, independently of its practical convenience, the best form of compound prism for general use.

Compound Prism.—We have here

$$\sin\phi=\mu\sin\phi'\,;\quad \mu'\sin\psi=\mu\sin\psi'\,;\quad \sin\chi=\mu'\sin\chi'\,;$$

$$\phi'+\psi'=\alpha\,;\quad \psi+\chi'=\beta\,;\quad D=\phi-\chi-\alpha+\beta\,;$$

the deviation D being considered positive when the pencil is bent from the refracting edge of the flint prism.

1. *Magnifying-power.* μ, μ' being constant,

$$m=\frac{\delta\chi}{\delta\phi}=\frac{\delta\phi'}{\delta\phi}\cdot\frac{\delta\psi'}{\delta\phi'}\cdot\frac{\delta\psi}{\delta\psi'}\cdot\frac{\delta\chi'}{\delta\psi}\cdot\frac{\delta\chi}{\delta\chi'}=\frac{\cot\phi}{\cot\phi'}\times-1\times\frac{\cot\psi'}{\cot\psi}\times-1\times\frac{\cot\chi'}{\cot\chi}$$

$$=\frac{\cos\phi}{\cos\phi'}\cdot\frac{\cos\psi'}{\cos\psi}\cdot\frac{\cos\chi'}{\cos\chi}=\frac{m''m'''}{m'},$$

where

$$m'=\frac{\cot\phi'}{\cot\phi}=\mu\,\frac{\cos\phi'}{\cos\phi}\,;\quad m''=\frac{\cot\psi'}{\cot\psi}=\frac{\mu}{\mu'}\frac{\cos\psi'}{\cos\psi}\,;\quad m'''=\frac{\cot\chi'}{\cot\chi}=\mu'\frac{\cos\chi'}{\cos\chi}.$$

2. *Dispersion.*—Taking logarithms and differentiating, μ and μ' being now variable,

$$\delta\phi=\frac{\delta\mu}{\mu}\tan\phi+\frac{\cot\phi'}{\cot\phi}\delta\phi'\,;\quad \delta\psi=\left(\frac{\delta\mu}{\mu}-\frac{\delta\mu'}{\mu'}\right)\tan\psi+\frac{\cot\psi'}{\cot\psi}\cdot\delta\psi'\,;$$

$$\delta\chi=\frac{\delta\mu'}{\mu'}\tan\chi+\frac{\cot\chi'}{\cot\chi}\,\delta\chi',$$

or

$$\delta\phi=\Delta'+m'\delta\phi'\,;\quad \delta\psi=\Delta''+m''\delta\psi'\,;\quad \delta\chi=\Delta'''+m'''\delta\chi'\,;$$

where

$$\Delta'=\frac{\delta\mu}{\mu}\tan\phi,\quad \Delta''=\left(\frac{\delta\mu}{\mu}-\frac{\delta\mu'}{\mu'}\right)\tan\psi,\quad \Delta'''=\frac{\delta\mu'}{\mu'}\tan\chi,$$

Δ', Δ'', Δ''' being the dispersion at the first, second, and third surfaces respectively; also

$$0=\delta\phi'+\delta\psi',\quad 0=\delta\psi+\delta\chi',\quad \text{and } \delta D=\delta\phi-\delta\chi.$$

Eliminating $\delta\phi'$ and $\delta\psi'$,

$$m''\delta\phi+m'\delta\psi=m''\Delta'+m'\Delta'';$$

and eliminating $\delta\psi$ and $\delta\chi'$, we have

$$m''m'''\delta\phi-m'\delta\chi=m''m'''\Delta'+m'm'''\Delta''-m'\Delta''',$$

with the three further relations,

$$\begin{aligned}\delta\phi-m'\delta\phi' &=\Delta',\\ \delta\psi-m''\delta\psi' &=\Delta'',\\ \delta\chi-m'''\delta\chi' &=\Delta'''.\end{aligned}$$

3. *Purity.*—Putting Π', Π'', Π''' for the purity of the first, second, and third surfaces respectively,

$$\Pi'=\frac{\Delta'}{m'},\quad \Pi''=\frac{\Delta''}{m''},\quad \Pi'''=\frac{\Delta'''}{m'''},$$

and we have

$$\Pi'=\frac{\delta\mu}{\mu}\tan\phi',\quad \Pi''=\left(\frac{\delta\mu}{\mu}-\frac{\delta\mu'}{\mu'}\right)\tan\psi',\quad \Pi'''=\frac{\delta\mu'}{\mu'}\tan\chi',$$

$$m''m'''\delta\phi-m'\delta\chi=m'm''m'''(\Pi'+\Pi'')-m'm'''\Pi'''.$$

Taking the three cases as before, we have :—

i. $\delta\phi=0$, $\therefore \Delta=m\Delta'+m'''\Delta''-\Delta'''=m''m'''(\Pi'+\Pi'')-m'''\Pi'''$

$$=m''m'''\left\{\frac{\delta\mu}{\mu}\tan\phi'+\frac{\delta\mu}{\mu}-\frac{\delta\mu'}{\mu'}\tan\ \psi'\right\}-m'''\frac{\delta\mu'}{\mu'}\tan\chi';$$

$$\Pi=m'\left(\Pi'+\Pi''-\frac{\Pi'''}{m''}\right).$$

ii. $\delta\phi'=-\delta\psi'=0$ and $\delta\psi=-\delta\chi'=0$,

$$\therefore \delta\phi=\Delta'=m'\Pi',\quad \delta\psi=\Delta''=m''\Pi'',\quad \delta\chi=\Delta'''=m'''\Pi''',$$

and $\delta D=\Delta'-\Delta'''=m'\Pi'-m'''\Pi'''$.

iii. $\delta D=0$, $\therefore \delta\phi=\delta\chi=$ angle through which prism is turned $=\theta$. Then

$$\theta=\frac{m\Delta'+m'''\Delta''-\Delta'''}{m-1}=\frac{\Delta}{m-1}=\frac{m}{m-1}\Pi.$$

Taking the three forms of compound prism, we have :—

I. *Half-prism magnifying.*

1. Magnifying-power $m_{,}=\dfrac{m''m'''}{\mu}=\dfrac{\cos\psi'}{\cos\psi}\cdot\dfrac{\cos\chi'}{\cos\chi}$.

2. Dispersion .... $\Delta_{,}=m'''\Delta''-\Delta'''=m_{,}\mu\left(\dfrac{\delta\mu}{\mu}-\dfrac{\delta\mu'}{\mu'}\right)\tan\psi'-\dfrac{\delta\mu'}{\mu'}\tan\chi'$.

3. Purity........ $\Pi_{,}=\mu\left(\Pi''-\dfrac{\Pi'''}{m''}\right)=\dfrac{\Delta_{,}}{m_{,}}$

II. *Half-prism diminishing.*

1. Magnifying-power . . $m_{,,} = \frac{1}{m_{,}}$.
2. Dispersion . . . . $\Delta_{,,} = \frac{\Delta_{,}}{m_{,}}$.
3. Purity . . . . $\Pi_{,,} = m_{,}\Pi_{,} = \Delta_{,}$.

III. *Isosceles prism.*

1. Magnifying-power . . $m_{,,,} = 1$.
2. Dispersion . . . . $\Delta_{,,,} = 2\Delta_{,}$.
3. Purity . . . . $\Pi_{,,,} = 2\Delta_{,}$.

In order to fix the ideas, let us take the same flint, as in the case of the simple prism, and for the crown glass $\mu_A = 1{\cdot}510$, $\mu_H = 1{\cdot}530$. Then

$$\frac{\delta\mu}{\mu} = \frac{1}{25},\ \frac{\delta\mu'}{\mu'} = \frac{1}{75},\ \text{and}\ \frac{\delta\mu}{\mu} - \frac{\delta\mu'}{\mu} = \frac{2}{75},\ \text{and}\ \mu = \frac{7}{4}\ \text{very nearly};$$

$$\therefore \Delta = -\frac{1}{150}\left\{7m \tan\psi' - 2\tan\chi\right\}.$$

The first term increases with ψ', or the angle of the half-prism of flint, which should therefore be as large as practicable, consistently with good definition and moderate loss of light: it also increases with m; but in that case the second term increases too in so far as m depends on χ. It will therefore be desirable to find the rate of variation of m with χ, supposing ψ and ψ' to remain constant. Since

$$m = \frac{\cos\chi'}{\cos\chi} \cdot \frac{\cos\psi'}{\cos\psi},$$

we have, taking logarithms of both sides and differentiating,

$$\frac{\delta m}{m} = \tan\chi\,\delta\chi - \tan\chi'\,\delta\chi';$$

whence, since

$$\cos\chi\,\delta\chi = \mu'\cos\chi'\,\delta\chi',\ \text{and}\ \frac{\tan\chi'}{\tan\chi} = \frac{1}{\mu'}\frac{\cos\chi}{\cos\chi'},$$

$$\delta m = m\tan\chi\left\{1 - \frac{1}{\mu'^2}\frac{\cos^2\chi}{\cos^2\chi'}\right\}\delta\chi$$

$$= m\tan\chi \cdot \frac{\mu'^2 - 1}{\mu'^2 - \sin^2\chi} \cdot \delta\chi,$$

which increases rapidly with χ, and

$$\delta \,.\, \tan\chi = \frac{\delta\chi}{\cos^2\chi}.$$

Taking as an example $\chi = 60°$ and $m = 3$, we have

$$\delta m = \frac{5}{2}\sqrt{3} \,.\, \delta\chi = 4{\cdot}3\delta\chi \ \text{and}\ \delta \,.\, \tan\chi = 4\delta\chi.$$

The following Tables are arranged in the same form as those for the simple prism, taking

$$\mu_A = 1{\cdot}700,\ \mu'_A = 1{\cdot}510,\ \text{and therefore}\ \frac{\mu_A}{\mu'_A} = 1{\cdot}130,$$

$$\mu_H = 1{\cdot}770,\ \mu'_H = 1{\cdot}530,\qquad \frac{\mu_H}{\mu'_H} = 1{\cdot}160.$$

Crown. Dispersion and Magnifying-power.

χ'.	χ_H.	χ_A.	$\frac{m_H'''}{\mu_H} = \frac{\cos\chi'}{\cos\chi_H}$.	$\frac{m_A'''}{\mu'_A} = \frac{\cos\chi'}{\cos\chi_A}$.
0°	0° 0′	0° 0′	1·000	1·000
4	6 8	6 3	1·003	1·003
10	15 26	15 14	1·022	1·021
14	21 44	21 26	1·044	1·035
18	28 13	27 49	1·080	1·076
22	35 1	34 29	1·132	1·125
26	42 9	41 25	1·210	1·199
28	45 55	45 9	1·270	1·253
30	49 54	49 2	1·345	1·320
31	52 0	51 3	1·393	1·364
32	54 11	53 8	1·450	1·413
33	56 26	55 20	1·518	1·474
34	58 47	57 35	1·600	1·547
35	61 21	60 0	1·708	1·638
36	64 2	62 33	1·847	1·755
37	67 2	65 21	2·047	1·915
38	70 18	68 21	2·338	2·136
39	74 20	71 51	2·880	2·495
40	79 32	76 9	4·210	3·210
41	..	82 7	..	5·510

Light Reflected.

χ'.	Polarized		Total.
	in plane of incidence.	in perpendicular plane.	
	per cent.	per cent.	per cent.
0°	4·6	4·6	4·6
10	5·1	4·2	4·7
18	6·3	3·2	4·8
26	9·4	1·4	5·4
30	12·3	0·5	6·4
34	H 17·6	0·0	8·8
	A 16·0	0·0	8·0
36	H 22·7	0·9	11·8
	A 20·5	0·6	10·6
38	H 31·6	4·4	18·0
	A 27·7	3·0	15·4
39	H 40·7	9·4	25·1
	A 33·6	6·0	19·8
40	H 53·4	21·7	37·6
	A 43·2	12·8	28·0
41	A 61·7	32·6	47·2

The dispersion is simply the difference between corresponding quantities in the two columns χ_H and χ_A.

The magnifying-power (given in the last two columns for H and A) in this and following Tables is to be understood as that of a half-prism having χ' (or ψ') for its refracting angle.

Flint to Crown.

Dispersion and Magnifying-power.

ψ'.	ψ_H.	ψ_A.	$\psi_H - \psi_A$.	$\frac{\mu'}{\mu} m_H'' = \frac{\cos\psi'}{\cos\psi_H}$.	$\frac{\mu'}{\mu} m_A'' = \frac{\cos\psi'}{\cos\psi_A}$.
°	° ′	° ′	° ′		
45	55 6	53 2	2 4	1·24	1·18
47	58 1	55 44	2 17	1·29	1·21
49	61 6	58 31	2 35	1·36	1·26
51	64 21	61 26	2 55	1·45	1·32
53	67 52	64 29	3 23	1·60	1·39
55	71 50	67 46	4 4	1·84	1·52
56	74 5	69 32	4 33	2·04	1·60
57	76 35	71 23	5 12	2·35	1·71
57½	78 3	72 22	5 41	2·60	1·77
58	79 38	73 24	6 14	2·95	1·86
58½	81 30	74 28	7 2	3·53	1·95
59	83 53	75 37	8 16	4·82	2·07
59½	88 10	76 49	11 21	15·86	2·23

Light Reflected.

ψ'.	Polarized		Total.
	in plane of incidence.	in perpendicular plane.	
	per cent.	per cent.	per cent.
45°	3·2	0·1	1·6
49	5·0	0·6	2·8
53	H 8·9	2·5	5·7
	A 5·0	1·1	3·1
55	H 13·1	5·1	9·1
	A 6·9	2·1	4·5
56	H 16·4	7·6	12·0
	A 8·3	3·0	5·7
57	H 21·4	11·5	16·5
	A 10·0	4·1	7·1
58	H 29·9	18·9	24·4
	A 12·5	5·9	9·2
59	H 48·6	37·5	43·1
	A 16·1	8·6	12·4
59½	H 80·4	74·6	74·5
	A 18·6	10·7	14·7

For the purpose of comparing the direct-vision and ordinary compound prism, we may take as a particular case $\alpha=56^\circ$, and $\beta=106^\circ\,32'$ for the first, and $37^\circ\,38'$ for the second, so that the extreme angle of incidence or emergence at the crown will be the same in both forms, viz. 65° 21′.

We thus have

Direct-vision Half-prism.		*Compound Half-prism.*	
$\alpha=56^\circ\;0'$	$\beta=106^\circ\;32'$	$\alpha=56^\circ\;0'$	$\beta=37^\circ\;38'$
$\chi_A=65\;\;21$		$\chi_A=52\;\;56$	
$\chi_H=55\;\;11$		$\chi_H=65\;\;21$	
Dispersion $=10\;\;10$		Dispersion $=12\;\;25$	
Deviation			
for A $=-14\;\;49$,		for A $=+34\;\;34$,	
for H $=-\;\;4\;\;39$,		for H $=+46\;\;59$,	

the deviation being considered positive when it is in the same direction as that of the flint prism.

Magnifying-power.

Direct-vision Half-prism.	*Compound Half-prism.*
$m_A=3{\cdot}07$	$m_A=2{\cdot}26$
$m_H=3{\cdot}04$	$m_H=3{\cdot}96$

Irrationality of Dispersion.

$$\delta\chi_A=-\frac{1}{150}(31{\cdot}8-4{\cdot}4)\qquad \delta\chi_A=-\frac{1}{150}(23{\cdot}4+2{\cdot}6)$$

$$=-{\cdot}183\text{ or }-10^\circ\,26'\qquad =-{\cdot}173\text{ or }-9^\circ\,55'$$

$$\delta\chi_H=-\frac{1}{150}(31{\cdot}5-2{\cdot}9)\qquad \delta\chi_H=-\frac{1}{150}(41{\cdot}0+4{\cdot}4)$$

$$=-{\cdot}191\text{ or }-10^\circ\,57'\qquad =-{\cdot}303\text{ or }-17^\circ\,22'.$$

Light lost by Reflexion.

	Polarized		Total.	Polarized		Total.
	in plane of incidence.	in perpendicular plane.		in plane of incidence.	in perpendicular plane.	
	per cent.	per cent.	per cent.	per cent.	per cent.	per cent.
For A	36·5	11·3	23·9	27·1	10·4	18·8
For H	34·7	15·3	25·0	44·1	15·6	29·9

The advantage of the direct-vision prism is thus clearly shown, whether as compared with a compound prism of the other form, or with two simple prisms of the same kind of flint and giving the same dispersion.

In fact the direct-vision half-prism here considered gives nearly the same dispersion as a simple prism of 64°, with less loss of light and without the great irrationality which makes the dispersion in the other case merely illusory.

It is to be remarked that there still remains the irrationality in the variation of the *refractive index for glass,* which may, however, be partially corrected in the direct-vision prism by increasing the angle of the crown. The prism would, however, no longer be direct-vision, even for the ultra-violet, the deviation being negative and considerable in amount for the visible rays.

So far the case has been considered in which the pencils for different parts of the spectrum all pass in the same direction through the prism (parallel to the base), and the collimator and telescope are moved; but it is also possible to keep the collimator and telescope fixed and to turn the prism merely. In the former plan, which is that usually adopted, the limit to the angle of the prism is fixed by the condition that the angle of incidence or emergence for the violet at the oblique face must not be too large, or, in other words, that the angle of refraction must not be very near the critical angle for the violet, which is smaller than that for the red; and consequently great dispersion is accompanied with corresponding irrationality, except in the case of the direct-vision prism. If, however, the deviation be kept the same for all parts of the spectrum—which can be done by turning the prism (provided it be not isosceles)—this objection is got rid of, and at the same time the construction of the spectroscope is made much more simple. The refracting angle of a half-prism, whether simple or compound, can thus be increased, so as to give the most suitable dispersive power for the middle of the dispersion spectrum (about F) when the pencil is incident perpendicularly on the first face. By turning the prism successively in one direction or the other, so as to increase the angle of refraction for the red or diminish it for the violet, the different parts of the spectrum can be made to emerge successively in the same direction, and the dispersive and magnifying-powers, as well as the loss of light, will be sensibly constant throughout the spectrum, the dispersion at the first face being extremely small. Of course, if the whole spectrum be included in the field of view there will be the usual inequality of scale; but such an extent of field can only be used for viewing a spectrum with small dispersion; and for purposes of measurement, the part examined must be brought to the middle of the field. It may be remarked that the dispersion may be reduced to about one half by turning the half-prism till the angles of incidence and emergence become equal, when it acts as an isosceles prism at minimum deviation. This property is useful for the examination of faint objects, the decrease in dispersion being further accompanied by an increase in the transmitted light. The spectrum, however, would in this case be thrown some 3° out of the centre of the field, and a motion either of the viewing-telescope or of the eyepiece would be necessary.

In fig. 3 is shown the course of a pencil of red rays (full lines) and of violet rays (dotted lines) within a direct-vision half-prism as it is turned about a centre in the manner just described. The rays, whether red or violet, emerge parallel to their original direction, the diagram representing the course relative to the prism as it is turned.

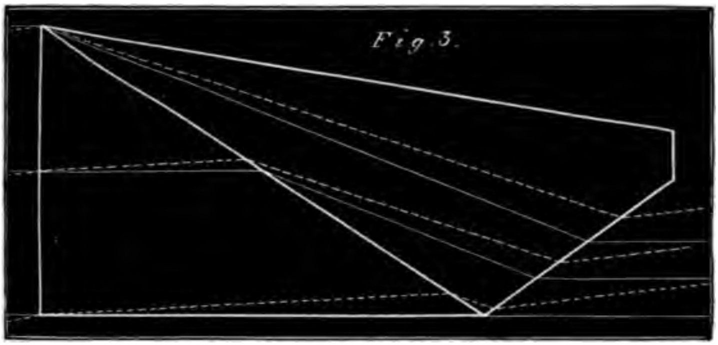
Fig. 3.

Taking the case of a single half-prism with fixed collimator and spectroscope, we may increase the refracting angle (or the angle of refraction for mean rays), and thus obtain much larger dispersion. In comparing the three forms of prism (simple, direct-vision, and ordinary compound), the irrationality was the most important consideration; but this does not here enter into the question. The direct-vision prism, however, has still the advantage, as compared with the compound prism, of giving a more convenient form of spectroscope, whilst, as we can now use very deep angles for the flint, the dispersion at the exterior surface of the crown hardly affects the resulting dispersion.

As compared with a simple half-prism of flint, there is the advantage of being able to make ψ' (or the angle of the flint) nearly twice as large, with a corresponding increase in the purity; for it will be remarked that although the magnifying-power and dispersion may be increased almost indefinitely, there is a limit to the purity given by making $\psi' =$ the critical angle; and consequently there will be great advantage in a combination of flint and crown surfaces which increases the critical angle without materially diminishing the dispersive power.

In illustration of this the following Table may be interesting. It is calculated for a specimen of very dense flint glass (spec. grav. 5·0) of remarkable purity, which Mr. Hilger has procured, and with which he has made to my instructions several experimental direct-vision half-prisms. The refractive indices of this glass for A and H are $\mu_A = 1·7595$ and $\mu_H = 1·8411$.

$\psi' = a$.	ψ_A.	$\frac{\mu'}{\mu} m_A''$.	$D'' = -D'''$.	χ'_A.	χ_A.	β.	m.	Π.	Δ.
54°	70° 37′	1·77	16° 37′	27° 25′	44° 2′	98° 2′	2·19	4° 43′	10°·3
55	72 47	1·94	17 47	28 41	46 28	101 28	2·43	4 49	11·7
56	75 10	2·19	19 10	30 8	49 18	105 18	2·89	4 55	14·2
56½	76 29	2·36	19 59	30 57	50 56	107 26	3·21	5 1	16·1
57	77 55	2·61	20 55	31 49	52 44	109 44	3·78	5 7	19·3
57½	79 33	2·97	22 3	32 46	54 49	112 19	4·56	5 14	23·8
58	81 27	3·56	23 27	33 53	57 20	115 20	5·50	5 20	29·4
58½	83 49	4·84	25 19	35 13	60 32	119 2	8·35	5 26	45·5
59	88 0	14·76	29 0	*37 20*	66 20	125 20	29·20	5 32	162·6

This Table gives the angles of refraction and emergence and the deviations at the two refractions (D″, D‴), and also the magnifying-power, purity, and dispersion for various direct-vision half-prisms, α being the angle of the flint and β the angle of the crown.

The light lost by reflexion at the refraction from flint to crown is as follows:—

Light lost by Reflexion.

ψ′ = α.	Polarised		Total.
	in plane of incidence.	in perpendicular plane.	
°	per cent.	per cent.	per cent.
54	12·1	4·2	8·2
56	19·1	9·2	14·2
56½	22·0	11·5	16·8
57	25·4	14·5	20·0
57½	30·4	18·9	24·7
58	37·7	25·7	31·7
58½	49·2	37·5	43·4
59	79·1	72·9	76·0

As the amount of light lost in different combinations of prisms requires careful consideration, I have by the help of the preceding Tables calculated the loss in passing through trains of half-prisms, having respectively angles of 57° for the flint, with 110° for the crown, and 58½° for the flint, with 120° for the crown. These particular forms of half-prism (which I will call A and B) have been used in two experimental spectroscopes to be described shortly, and I have therefore selected them as examples.

Light lost by Reflexion.

	Polarized.		Total.	Dispersion.
	in plane of incidence.	in perpendicular plane.		
	per cent.	per cent.	per cent.	°
One half-prism A ..	40·9	21·2	31·0	19·3
Two half-prisms A .	65·1	37·9	51·5	73·0
Three ,, ,, ..	79·4	51·1	65·2	276·0
Four ,, ,, ..	87·8	61·4	74·6	1043·0
One half-prism B ..	61·7	42·5	52·1	45·5
Two half-prisms B .	85·3	66·9	76·1	380·0

It will be seen that for equal dispersion the loss of light is slightly greater for a train of half-prisms B; but this is fully compensated for by *the diminished* absorption, as fewer prisms will be required.

In considering the most advantageous combination of prisms in a spectroscope, it will be necessary to bear in mind the different purposes for which the instrument is used. The requirements in a spectroscope may thus be divided into the following heads:—

(1) Wide separation of the lines, with little loss of light and moderate purity in the spectrum.

(2) Great purity in the spectrum.

(3) Wide separation of the lines accompanied by considerable purity.

In (2) and (3) loss of light is of secondary importance. For measuring the displacement of lines in the spectra of stars and work of a similar character (1) must be specially considered; and here the width of the slit is determined by the size of the star's image rather than by the angle subtended at the object-glass of the collimator. With prisms of fair size sufficient purity will usually be obtained for measurement of the strong lines without limiting the slit further; the most important point is large aperture in the equatoreal, as the amount of light collected varies with the aperture. This will readily be seen by considering that the angle subtended by the diffraction image of a star at the optical centre of the object-glass (or mirror) varies inversely as the aperture; hence for prisms of a given size the purity of the spectrum will be the same for all apertures, if the focal length of the collimator correspond.

For a bright-line spectrum, such as that of the solar prominences, which is seen projected on the continuous spectrum of the sky and of which the visibility depends on the contrast with the background and not on the absolute brightness, (2) is of the greatest importance; whilst special attention must be paid to (3) in the case of the sun.

We may compare three arrangements of prisms:—(1) Half-prisms magnifying; (2) Half-prisms diminishing; (3) Whole prisms; but in doing this we must take account of the width of the slit and the breadth of the pencil as determining the purity of the spectrum and its brightness. At the same time, as the size of the prisms practically fixes the limit to the power of the spectroscope, the quantity of glass used in the different cases should also be borne in mind. This latter consideration is important, because in a train of half-prisms magnifying the breadth of the pencil is diminished in the ratio $1 : m$ with each half-prism (the height remaining unaltered), and the reverse takes place with a train of half-prisms diminishing. Thus in both cases the half-prisms may be made narrower and narrower in geometrical progression, starting with that end which begins with a perpendicular face. The difference between the two cases is, that in the first case the train (counting from the slit) begins with the broadest prism, and in the second case with the narrowest, so that with the latter arrangement we have a narrower pencil and consequently less light. In the case of a train of whole prisms the pencil is narrower than for the half-prisms magnifying, in the ratio of $1 : m$. The thickness of the glass through which the rays pass is another important consideration; and from this point of view *the half-prism train has* a great advantage. The breadth of the spectrum, as compared with the height of the slit, should also be taken into account, for this affects the brightness directly. Hitherto advantage has not been taken of the circumstance that the height of the slit is practically unlimited (except in the case of stars), and that therefore it is unnecessary to magnify the spectrum in this direction. Thus the advantage of the train of "half-prisms magnifying" is obvious, since the breadth of the spectrum is left unaltered. In spectroscopes as ordinarily constructed, the breadth of the spectrum is magnified at the same time as the separation of the lines, and thus a high magnifying-power is accompanied by a great decrease in the brightness, the latter varying inversely as the square of the power. By using a cylindrical eyepiece, however, this objection may be removed; and there would appear to be no practical difficulty in this plan, since little or no enlargement of the breadth of the spectrum is required. For bright-line spectra generally a Ramsden eyepiece with cylindrical lenses would answer the purpose well, the height of the slit being sufficient to give a convenient breadth to the spectrum without any amplification in this direction; but for viewing the solar prominences the magnifying-power should be the same in both directions; and thus with the train of "half-prisms diminishing," which, as will be seen, gives great purity in the spectrum with a corresponding decrease in the breadth of the image of the slit, the magnifying-power of the eyepiece must be high in the direction of the breadth of the slit, and low in the perpendicular direction. An eyepiece formed with a cylindrical concave or Barlow lens (within the principal focus of the viewing-telescope) and an ordinary convex field-lens in combination with a convex cylindrical eye-lens, would produce this effect, the axes of both cylindrical lenses being parallel to the slit.

The effect of such an eyepiece may easily be investigated.

Let f_1, f_2 be the focal lengths of the concave and convex cylindrical lenses, f the focal length of the convex field-lens, and F the focal length of the viewing-telescope; then, if M, M′ be the magnifying-powers parallel and perpendicular to the slit,

$$M=-\frac{F}{f}.$$

For the rays in the perpendicular plane, suppose the concave cylindrical lens to form an enlarged image on the field-lens, which will therefore produce no effect, this image being at the principal focus of the cylindrical eye-lens.

Then if v be the distance from the concave lens of the focus after refraction, and m' the enlargement of the image, f and v are both negative, and

$$\frac{v}{v-f}=m'\,; \quad \therefore\ v=\frac{m'}{m'-1}f, \quad v-f=\frac{1}{m'-1}f.$$

Also

$$\frac{1}{v}-\frac{1}{v-f}=\frac{1}{f_1}\,; \quad \therefore\ f_1=\frac{-m'}{(m'-1)^2}f,$$

and

$$M' = \frac{m'\,F}{f_2}.$$

If we make $f_2 = -f_1$ so as to equalize the two cylindrical lenses, we have $M' = -(m'-1)^2 \cdot \frac{F}{f}$. Thus if

$$M' = 9\frac{F}{f},\quad m'=4, \text{ and } f_1 = -f_2 = \tfrac{4}{9}f;$$

$$M' = 25\frac{F}{f},\quad m'=6, \text{ and } f_1 = -f_2 = \tfrac{6}{25}f.$$

Taking 1 inch as the equivalent focal length of an ordinary eyepiece, we should get in the perpendicular direction magnifying-powers 9 and 25 times as great with cylindrical lenses of $\frac{4}{9}$ and $\frac{6}{25}$ inch respectively. There would probably be no difficulty in applying such lenses.

We shall see, however, that one or more small half-prisms placed between the eye-lens and the eye would answer the same purpose, with the advantage of increasing the dispersion; and this arrangement appears to be decidedly superior to a cylindrical eyepiece.

We may now find expressions for the angular breadth of the lines, the dispersion and the purity of the spectrum in the different cases, as well as the quantity of glass used for the train of prisms and its total thickness.

Let $p=$ number of whole prisms.
$n=$ number of half-prisms.
$m=$ magnifying-power of each.
$2\Delta=$ dispersion of each whole prism.
$Q=$ quantity of glass in the smallest half-prism of the train.
$t=$ thickness of glass in the smallest half-prism of the train.
b_1, b_2, and $b_3=$ breadth of slit in the three cases.
$1-c=$ coefficient of absorption per unit of thickness.
$1-k_1$ and $1-k_2=$ proportion lost by reflexion in passing through one half-prism of the light polarized in and perpendicular to plane of incidence.
$1-k_1$ and $1-k_2=$ similar quantities for a whole prism.

1. *Half-prisms magnifying.*

By what precedes it will be clear that each half-prism in the train will magnify the angular separation of two pencils falling on it, whether coming from the two edges of the slit, or corresponding to two lines in the spectrum (such as the two D lines). Thus the breadths of the two D lines will be magnified in the same ratio as their separation, due to the dispersion of the preceding prisms, and to this will be added the dispersion proper (as distinct from the magnifying-power) of the half-prism itself.

Thus, there being n half-prisms in the train,

Breadth of spectrum-lines .. $=m^n \,.\, b_1$.

Dispersion .... $=(m^{n-1}+m^{n-2}+\,..\,+m^2+m+1)\Delta$

$$=\frac{m^n-1}{m-1}\,.\,\Delta$$

Purity of spectrum .... $=\dfrac{m^n-1}{m-1}\,.\,\dfrac{1}{m^n}\,.\,\dfrac{\Delta}{b_1}$.

Quantity of glass .... $=(m^{2(n-1)}+m^{2(n-2)}+\,..\,+m^4+m^2+1)Q$

$$=\frac{m^{2n}-1}{m^2-1}\,.\,Q.$$

Thickness of glass.......... $=(m^{n-1}+m^{n-2}+\,..\,+m^2+m+1)t$

$$=\frac{m^n-1}{m-1}\,.\,t.$$

Total brightness of spectrum.. $=b_1 \,.\, \frac{1}{2}(k_1{}^n+k_2{}^n)c^{\frac{m^n-1}{m-1}\,.\,t}$.

2. *Half-prisms diminishing.*

Breadth of spectrum-lines .. $=\dfrac{1}{m^n}\,.\,b_2$.

Dispersion .... $=\left(\dfrac{1}{m^{n-1}}+\dfrac{1}{m^{n-2}}+\,..\,+\dfrac{1}{m^2}+\dfrac{1}{m}+1\right)\dfrac{\Delta}{m}$

$$=\frac{m^n-1}{m-1}\,.\,\frac{1}{m^n}\,.\,\Delta.$$

Purity of spectrum .... $=\dfrac{m^n-1}{m-1}\,.\,\dfrac{\Delta}{b_2}$.

Quantity and thickness of glass the same as for "half-prisms magnifying."

Total brightness of spectrum.. $=\dfrac{b_2}{m^n}\,.\,\frac{1}{2}(k_1^n+k_2^n)c^{\frac{m^n-1}{m-1}\,.\,t}$.

3. *Whole prisms.*

Breadth of spectrum-lines .. $=b_3$.

Dispersion .... $=2p\,.\,\Delta$.

Purity of spectrum .... $=2p\,.\,\dfrac{\Delta}{b_3}$.

Quantity of glass .... $=2p\,.\,m^{2(n-1)}\,.\,Q$.

Thickness .... $=2p\,.\,m^{n-1}\,.\,t$.

Total brightness of spectrum.. $=\dfrac{b_3}{m}\,.\,\frac{1}{2}(k_1{}^p+k_2{}^p)c^{2pm^{n-1}t}$.

k_1 and k_2 are nearly equal to $k_1{}^2$ and $k_2{}^2$ respectively.

4. *A train of* n *half-prisms magnifying followed by a train of* n *half-prisms diminishing.*

Breadth of spectrum lines .. $=1$.

Dispersion=purity ...... $=(m^{n-1}+m^{n-2}+\,..\,+m+1)\,.\,\Delta\,.\,\frac{1}{m^n}$

$$+\left(\frac{1}{m^{n-1}}+\frac{1}{m^{n-2}}+\,..\,+\frac{1}{m}+1\right).\frac{\Delta}{m}$$

$$=2\,\frac{m^n-1}{m-1}\,.\,\frac{\Delta}{m^n}.$$

Total brightness of spectrum.. $=b_1\,.\,\tfrac{1}{2}(k_1^{2n}+k_2^{2n})c^{2\frac{m^n-1}{m-1}\,.\,t}.$

5. *A train of* n *half-prisms diminishing followed by a train of* n *half-prisms magnifying.*

Breadth of spectrum-lines .. $=1$

Dispersion=purity .... $=\left(\frac{1}{m^{n-1}}+\frac{1}{m^{n-2}}+\,..\,+\frac{1}{m}+1\right)m^n\Delta$

$$+(m^{n-1}+\,..\,+m+1)\Delta$$

$$=2\frac{m^n-1}{m-1}\,.\,\Delta.$$

Total brightness of spectrum.. $=\frac{b_2}{m^n}\,.\,\tfrac{1}{2}\,(k_1^{2n}+k_2^{2n})c^{2\frac{m^n-1}{m-1}t}.$

The second train of half-prisms in (4) and (5) may be placed between the eye-lens and the eye, and will then conveniently replace the cylindrical magnifier. The prisms may in that case be very small; their dispersion, however, will not be magnified by the eyepiece, and they will therefore add but little to the dispersion or purity.

In fact if M be the magnifying-power of the viewing-telescope, the dispersion would be $(\mathrm{M}+1)\frac{m^n-1}{m-1}.\frac{\Delta}{m^n}$ and $(\mathrm{M}+1)\frac{m^n-1}{m-1}.\,\Delta$, and the purity $\frac{\mathrm{M}+1}{\mathrm{M}}.\frac{m^n-1}{m-1}.\frac{\Delta}{m^n}$ and $\frac{\mathrm{M}+1}{\mathrm{M}}.\frac{m^n-1}{m-1}.\,\Delta$ for (4) and (5) respectively.

This is on the supposition that the second train has the same magnifying-power as the first.

Now for the same brightness of spectrum, neglecting loss of light by reflexion and absorption, we must have $b_2=m^nb_1$ and $b_3=mb_1$, the breadth of the slit being increased to compensate for the decrease in the breadth of the incident pencil. Where this is not practicable (as for star-spectra) the train of "half-prisms magnifying" has the advantage of greater brightness; and, as it gives at the same time wide separation of the lines, it best fulfils the condition (1).

On the other hand, for viewing the solar prominences, loss of light is of little or no consequence, the important point being to have a strong contrast between the monochromatic image of the slit (filled with the light of the prominence) and the dispersed image due to the light of the sky. Thus the breadth of the slit does not enter into the question, and great purity in the spectrum is the chief desideratum. A train of "half-prisms diminishing" is evidently by far the best arrangement; but for convenience in delineating the prominences, a cylindrical eyepiece or a train of half-prisms magnifying (between the eye-lens and the eye) must be used.

Such a combination is also suitable for spectrum-analysis, where a bright-line spectrum has to be distinguished from a continuous spectrum forming the background. Thus with this arrangement the sodium lines in the flame of an ordinary paraffin candle are seen with great brilliancy.

In general, however, a combination of wide separation, purity, and brightness is required; and here we must take the values for b_2 and b_3 given above in terms of b_1.

$$\left.\begin{array}{ll}\text{(1) and (2) give the same purity} & \dfrac{m^n-1}{m-1}\cdot\dfrac{1}{m^n}\cdot\dfrac{\Delta}{b_1}\\ \text{(3) gives the purity ....} & 2p\,\dfrac{1}{m_1}\cdot\dfrac{\Delta}{b_4}.\end{array}\right\}$$

If we take the same number of half-prisms in the three cases, $2p=n$; and (3) is superior in this respect to (1) and (2), the purity being proportional to $1+1+1+\ldots$ to n terms and to $1+\frac{1}{m}+\frac{1}{m^n}+\ldots$ to n terms in the two cases respectively.

But as the practical question is how to get the best result out of a given quantity of glass, we should divide the purity by the quantity of glass, and thus we get the ratio for what may be called the efficiency,

$$E_1=\frac{m+1}{m^n+1}\cdot\frac{1}{m^n}\cdot\frac{\Delta}{b_1}\text{ for (1) and (2),}$$

and

$$E_3=\frac{1}{m^{2n-1}}\cdot\frac{\Delta}{b_1}\text{ for (3).}$$

Hence

$$\frac{E_1}{E_3}=\frac{m^n+m^{n-1}}{m^n+1}=1+\frac{m^{n-1}-1}{m^n+1},$$

which shows the advantage of the half-prism train, m being greater than 1.

On the other hand, there is a small additional loss of light at the perpendicular faces in the half-prism train amounting to 6 or 7 per cent. for each half-prism; but as not more than three, or at the most four, are ever likely to be required, even for the sun, this loss is hardly sensible.

As regards the *thickness* of glass, the ratio of purity to thickness of glass is the same in all three cases, viz.:—

$$\frac{1}{m^n}\cdot\frac{\Delta}{b}\cdot\frac{1}{t}.$$

We may consider one other point, viz. the effect of error in the surfaces and of unequal density in the glass, though the question is a very obscure one. An error in the surface being equivalent to a slight deviation in the incident ray, its effect will vary as the magnifying-power; and thus with the train of half-prisms magnifying, the errors of the half-prisms are magnified in proportion to their distance from the end of the train in the same ratio as the dispersions; and if there is no tendency to a repetition of similar errors throughout the train, these must be combined according to the ordinary law of errors of observation, and thus we should get probable error from whole train $=\sqrt{\frac{m^{2n}-1}{m^n-1}}\times$ probable error from one half-prism.

The train of "half-prisms diminishing" would give the same result, since a magnifying-power of m^n must be used to give the same separation. In the case of the whole prisms, a magnifying-power of $\frac{m^n-1}{m-1}\cdot\frac{1}{p}$ is required, and therefore probable error from whole train $=\frac{m^n-1}{m-1}\cdot\frac{1}{2\sqrt{p}}$ $\times$ probable error from a whole prism. Thus, considering that a higher magnifying-power must be applied where few prisms are used, the case of a long train of prisms would be equivalent to the arithmetical mean of a large number of fallible surfaces; but, independently of the fact that after a very moderate number the gain in accuracy of definition is very slight, practical experience seems to show that, owing probably to systematic errors in the surfaces or the glass, an increase in the number of prisms is accompanied by a corresponding loss of definition, and that consequently the law of errors does not apply. Hitherto a long train of prisms has been considered necessary, partly because it has been the only means of obtaining purity, *i. e.* dispersion as distinct from mere separation, and partly because the use of high magnifying-powers involved the weakening of the light by increase in the apparent breadth of the spectrum. As regards the effect of air-bubbles and other defects in the glass in stopping out light, it falls under the head of absorption, and constitutes a very serious objection to the long train of prisms.

Probably, from want of proper data, the effect of absorption in spectroscopes would appear to have been generally overlooked; and in many cases where compound prisms are employed the brightness of the spectrum is far inferior to what would have been obtained with much smaller prisms. Even with a train of simple prisms the loss by absorption is very great, and to this is superadded the loss by reflexion at the numerous surfaces, so that the disadvantages of a long train are sufficiently obvious. To obtain some information on this point, I have made several series of measures of the brightness of the light transmitted through various thicknesses of different kinds of glass, using a polarizing photometer and a chromatometer (on the principle of Prof. Clerk-Maxwell's colour-box); and though the observations I have made are not so extensive as I could wish for the elucidation of this interesting physical question, they are amply sufficient for the immediate object in view, showing conclusively the wasteful loss of light in the ordinary construction of the spectroscope. There would seem to be great differences in the absorptive power of different specimens of glass, depending on the manufacture rather than on the density, though all the specimens of very dense flint which I have examined absorb the blue and violet rays strongly, whilst crown glass absorbs the red and green. Notwithstanding its yellow colour, very dense flint (having a specific gravity of 5·0), when well made, actually transmits considerably more light than the specimens of crown I have met with. Thus I found that for a thickness of four inches the proportion of the incident light transmitted in the case of flint (sp. gr. 5·0) and crown was as follows, allowance being made for the loss by reflexion at the surfaces:—

	Red. per cent.	Green. per cent.	Blue. per cent.
Flint	56	49	5
Crown	40	39	21

From these results it would appear that a thickness of 4 inches of flint or 3 inches of crown would absorb half of the incident light, the glass being in each case well made.

The absorption in the case of a train of prisms can easily be calculated as follows:—

Let e^{-ft}, $e^{-ct'}$ be the coefficient of light transmitted through thicknesses t, t' of flint and crown.

h, k the height and breadth of the half-prism.

κ the abscissa for any ray measured from the edge of the flint prism.

$t = q\kappa$, $t' = q'(k-\kappa)$, where $q = n\tan\alpha$, $q' = \dfrac{n\sin\beta}{\cos\alpha\cos(\beta-\psi)}$, n being the number of half-prisms.

Putting B = Brightness of transmitted pencil, which is supposed to be large enough to fill the whole of the prism, we have

$$B = Ch\int_0^k e^{-ft}e^{-ct'}\,d\kappa = Ch\int_0^k e^{-(fq-cq')\kappa - c'q'k}\,d\kappa$$

$$= \frac{Ch}{-fq+cq'}\cdot\{e^{-fqk} - e^{-cq'k}\}, \text{ C being a constant.}$$

For evaluating this expression the following Table of the values of the negative exponential will be found convenient:—

·95	$e^{-0·051}$	·75	$e^{-0·288}$	·55	$e^{-0·598}$	·35	$e^{-1·050}$	·15	$e^{-1·897}$
·90	$e^{-0·105}$	·70	$e^{-0·357}$	·50	$e^{-0·693}$	·30	$e^{-1·204}$	·10	$e^{-2·303}$
·85	$e^{-0·162}$	·65	$e^{-0·431}$	·45	$e^{-0·798}$	·25	$e^{-1·386}$	·05	$e^{-2·996}$
·80	$e^{-0·223}$	·60	$e^{-0·511}$	·40	$e^{-0·916}$	·20	$e^{-1·609}$	·02	$e^{-3·912}$
								·01	$e^{-4·605}$

1. *Simple Prism.*

$$q' = 0 \qquad B = \frac{Ch}{fq}\cdot\{1 - e^{-fqk}\};$$

thus B increases with k, rapidly at first, and then more and more slowly.

Taking a prism of 60°, and putting $f = 0·173$, which would give an absorption of 50 per cent. for 4 inches, we have the following Table for the brightness $\frac{1}{n}\{1 - e^{-fqk}\}$ (omitting the constant factor $\frac{Ch}{2f\tan\alpha}$):—

Slant side	½ in.	1 in.	2 in.	3 in.	4 in.	6 in.	∞
No. of Prisms.	Brightness.						
1	·084	·16	·29	·40	·50	·65	1·00
2	·080	·145	·25	·325	·375	·44	0·50
4	·073	·125	·19	·22	·235		0·25
8	·063	·095	·118				0·125
16	·048	·059	·062				0·063

These figures show how little is gained by increasing the *breadth* of the prisms in a long train; in fact the

$$\text{Brightness} = \frac{\text{Absorption through the bases of all the prisms}}{\text{Number of prisms}};$$

and this absorption, as it approaches its limiting value 1, increases very slowly with the thickness of glass when that exceeds 6 or 8 inches. The *height* of the prisms may, however, be increased with advantage up to the limit determined by the condition that the height of the emergent pencil (after passing through the eyepiece) shall not exceed the diameter of the pupil of the eye. Thus with a magnifying-power of 10 on the viewing-telescope it is useless to make the prisms more than 2 inches in height.

The coefficient $\frac{Ch}{2f\tan\alpha}$ shows the variation of the brightness with the absorptive power of the glass and the angle of the prism.

The above formula has been obtained on the supposition that the whole of the prism is utilized right up to the refracting edge, which is assumed to have no sensible thickness. In practice, however, this is rarely the case, and the most valuable part of the prism is lost, from the difficulty of working a surface true up to the edge. In a long train of prisms this is a matter of some importance, where light is of consequence; and there is in such a train the further objection, that the pencil falling on the viewing-telescope is in fact rendered excentrical, and its effective breadth diminished, by the greater absorption in passing through the thick parts of the prisms.

2. *Compound Prisms.*

Here, as the breadth of the prism is increased, there will be a point where the increased absorption makes up for the increased brightness of the incident pencil; and when this is the case

$$\frac{dB}{dk} = 0, \text{ or } \frac{Ch}{fq - cq'}\{fqe^{-fqk} - cq'e^{-cq'k}\} = 0,$$

$$\therefore fqe^{-fqk} = cq'e^{-cq'k},$$

whence

$$k = \frac{1}{-fq + cq'} \cdot \log \frac{cq'}{fq};$$

suppose $fq=xcq'$,

$$\therefore\ k=\frac{1}{(x-1)cq'}\,.\log x,$$

which gives the limiting breadth k.

The condition $e^{-cq'k}=xe^{-fqk}$ may be expressed in words thus:—When the brightness is a maximum, the light transmitted through the thickest parts of the train of crown prisms must be x times that through the thickest parts of the train of flint prisms, x being the thickness of crown (expressed in terms of the total thickness corresponding to 1 inch breadth of prism) which gives the same absorption as the total thickness of flint corresponding to 1 inch breadth. The thickness is in each case that traversed by the ray.

When $x=1$ or $fq=cq'$, the formula becomes indeterminate; the absorption is then the same for all rays of the pencil, and

$$\mathrm{B}=\mathrm{C}hke^{-fqk}=\mathrm{C}hke^{-cq'k}.$$

Putting $\frac{d\mathrm{B}}{dk}=0$ as before, we have

$$k=\frac{1}{fq}\text{ or }\frac{1}{cq'};$$

or, when the brightness is a maximum, the breadth of the prisms must be such that the whole absorption is $\frac{1}{e}$, the absorption in passing through the crown being supposed equal to that in the flint. This will be approximately true in the case of the direct-vision prism, for which therefore the thickness of glass traversed (flint and crown combined) should not exceed 5·8 inches, if we take $f=\cdot 173$, as found by experiment for a very good specimen of flint. With less pure glass, such as is frequently used, this limiting thickness may be much smaller; in one piece of very dense flint which I examined it would be only 1·5 inch; but the absorption of the crown being here considerably less than that of the flint, the formula just given would no longer apply. Further, for the blue end of the spectrum f would be much larger, being, in fact, about ·78 for the yellow flint, whilst c is about ·34 for the crown, so that the total thickness for photographic work should certainly not exceed 3 inches, the limit being fixed by the absorption of the crown.

These numerical results, however, are given more as illustrating the principles on which spectroscopes should be constructed than as general rules applicable in all cases; for there is such great diversity in glass that, by a judicious selection, it is quite possible that a considerably greater thickness might prove to be admissible for the photographic rays. Further, the observations on which the values of the coefficients of absorption are based are liable to a small uncertainty on account of the difficulty of allowing for want of perfect polish in the two surfaces of the plate. They may be taken, however, as useful approximations to the truth.

From what precedes it will be seen that loss of light is so inseparably associated with dispersion, whether by making it necessary to use a narrower pencil or in other ways which are not so easily reduced to law, that there is theoretically but little to choose between different arrangements of prisms, and in particular between a long train of prisms and a short train of half-prisms; but practically the advantage seems to be altogether with the short train, which is less complicated and more convenient. The difficulties of the adjustment of a long train of prisms are so great that in practice the efficiency secured appears to fall very far short of the theoretical limit, and thus so-called powerful spectroscopes are a constant source of trouble and disappointment to those who use them. Even if once properly adjusted it is practically impossible to keep them so, and the consequence is that the sodium lines, for example, are seen hardly any better with eighteen or twenty prisms than with four or five.

The general conclusions arrived at with regard to the best combination of prisms may be summed up as follows:—

1. The direct-vision prism (as ordinarily used) possesses great advantages over other forms, simple or compound, especially in its correction of the irrationality of dispersion.

2. This irrationality can, however, be corrected in any form of "half-prism" by keeping the collimator and telescope relatively fixed, whilst the prism only is turned to bring successive parts of the spectrum into the field of view, and thus an angle approaching closely to the critical angle may be advantageously used.

3. A train composed of three or at most four "half-prisms (direct-vision) magnifying" is the best arrangement for giving wide separation of the lines in the solar spectrum, whilst one or, in some cases, two half-prisms similarly placed offer special advantages in the case of star-spectra. A well-worked slit will be required for examination of the finer lines in the sun's spectrum, and it would be well to use a concave or "Barlow" lens in the collimator so as to increase its effective focal length.

4. A train of two or three "half-prisms (direct-vision) diminishing," in combination with an eyepiece train of "half-prisms magnifying," is peculiarly adapted for the delineation of the solar prominences, and generally for the examination of bright-line spectra with a continuous background.

Mr. Hilger, who has entered with the greatest zeal into the practical application of these principles, and to whose skill the success of the idea is largely due, has made a direct-vision spectroscope for the Royal Observatory on the following plan (see fig. 1), which has been approved by the Astronomer Royal, and which embodies the above conclusions. The train consists of three half-prisms direct-vision for the red rays, each having an angle of 57° 0′ for the flint (sp. gr. 5·0) and 110° for the crown, and giving a dispersion of 20° with a magnifying-power of 3·8. The course of the rays is sufficiently indicated in the diagram, from which it

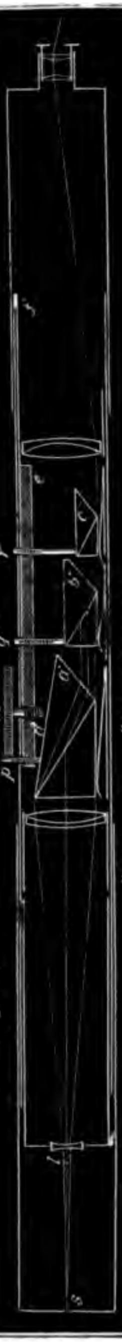

Fig. 1.

"Half-prism" Spectroscope

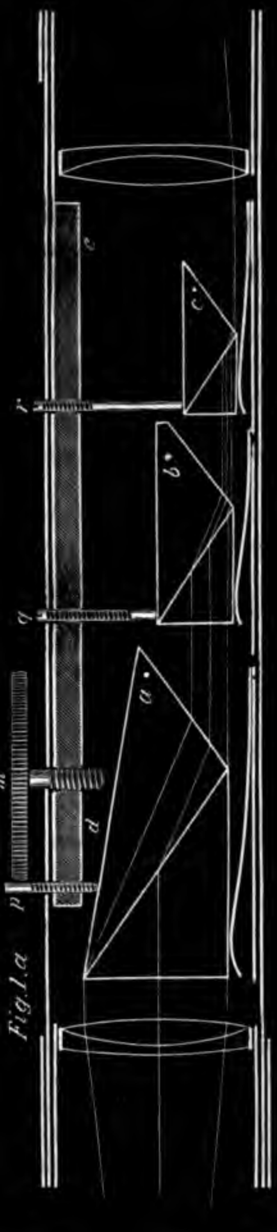

Fig. 1. a

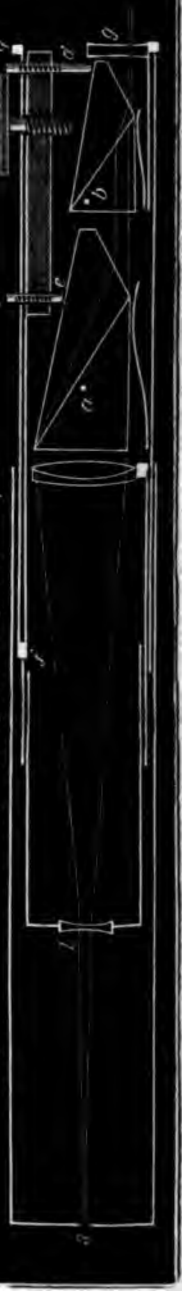

Fig. 2.

Miniature "Half-prism" Spectroscope

will be seen how the breadth of the pencil decreases, the rays being thrown to one side, as it were, by the refraction of the prisms. The prisms turn about centres *a*, *b*, *c*, so placed that the rays for different parts of the spectrum, after emerging from one prism, always fall on the same part of the face of the next prism, each being turned through the same angle. There is no difficulty in calculating the positions of these points in any particular case from the condition that $\delta\phi = \delta\chi$ as the prism is turned, and that the lateral displacement of the middle ray in one prism must be equal and opposite to the lateral motion of the perpendicular face of the next prism. For the sake of convenience in mounting (the centres being in the glass itself) the second and third prisms have been made larger than is absolutely necessary; and besides this a large portion of the crown might be cut off as far as optical requirements are concerned. The prisms are each turned through the same angle by the lever *d e f*, having its fulcrum at *f* and moved by the micrometer-screw *m*.

This lever carries the three adjustable screws *p*, *q*, *r*, so placed as to give the same angular motion to each prism, which is kept in contact with the corresponding screw by a spring on the opposite side. Thus a simple motion of the lever by means of the micrometer-screw brings different parts of the spectrum into the field; and this automatic motion will not be deranged in the least by withdrawing either one or two of the prisms, which are carried in short adapter tubes to admit of easy removal. The slit and eyepiece can readily be interchanged if great purity in the spectrum is desired; and, as just intimated, either one, two, or three half-prisms can be used, giving a dispersion or purity (as the case may be) of four, fifteen, or sixty ordinary prisms of 60°. The concave lens *l* has the effect of increasing the focal length of the collimator, and consequently of reducing the apparent breadth of the spectrum-lines for a given width of slit. At the same time the spectrum is made narrower and brighter; if, however, the spectroscope be applied to a large telescope, there is in fact no gain in light, a portion only of the pencil from the object-glass being used, whilst the width and height of the slit may be increased in the same proportion so as to keep the purity and breadth of spectrum constant. Notwithstanding this there is a practical advantage in the diminished effect of dust or irregularities on the slit. It remains to mention one important point in the direct-vision spectroscopes. Since the collimator and telescope are in a straight line, an image of the slit is formed in the centre of the field by rays which pass through the object-glasses above and below the prisms; and this serves as a bright reference line, which has the great advantage of being affected in the same way as the spectrum-lines by any shift of the collimator or viewing-telescope. Either a positive or negative eyepiece may be used with this ghost, and its brightness or colour may be varied by diaphragms or coloured glasses. Thus a convenient bright-line micrometer is formed, supplying a want which has long been felt by spectroscopists.

It may be remarked that this bright line gives a ready means of adjusting the collimator and telescope to focus, as, when this has been done, it should be seen distinctly at the same time as the spectrum-lines.

The success of this spectroscope has far exceeded all my anticipations. Though the first prism is only 0·75 inch in cross section, and the total length 11 inches, the power is far greater than that of any spectroscope with which I am acquainted, and this is accompanied by remarkable brightness in the spectrum. I have compared it carefully with the large spectroscope of the Royal Observatory, which has a train of ten compound prisms, and find that even with two prisms in the small direct-vision spectroscope the sodium lines are fully as widely separated as, and far better defined than, in the large spectroscope with its full power, whilst their brightness in the direct-vision spectroscope is incomparably greater. With the full train of three prisms this separation is quadrupled nearly, and the sodium lines are seen fully 3° apart with a power of 9 on the viewing-telescope. The compactness of the half-prism spectroscope, the simplicity of its construction, and the ease with which it is manipulated appear to leave little or nothing to be desired.

I have, however, tried a still more portable form of spectroscope, which for many purposes seems fully equal to the larger form. It is on the plan of the miniature spectroscopes, in which the eye takes the place of the viewing-telescope; and its general construction will be readily seen from fig. 2, which represents its actual size.

The prisms turn about centres *a*, *b*, being moved by the lever *d e f*, on which a micrometer-screw *m* acts as in the larger spectroscope; but to allow of more motion in the first half-prism the positions of the centres *a*, *b* have been altered, which can be done when there are only two prisms in the train, the condition between the lateral displacements still holding. The other arrangements are as before, the prism *b* being mounted in a small tube so as to be readily withdrawn if large dispersion is not required; but in place of the viewing-telescope and eyepiece, there is simply an eyeglass, *g*, adapted to the sight of the observer for distant objects, so that the rays pass through the prisms in a state of parallelism. The bright line formed by the image of the top and bottom of the slit is here seen above and below the spectrum, and measurements are made with great convenience.

As the whole of the magnifying-power has to be supplied by the prisms, I have increased the angle of the flint to 58° 30′, which approaches very closely to the critical angle; whilst in order to preserve the direct-vision form, the angle of the crown is 120°, giving a magnifying-power of 9 and a dispersion or purity (as the case may be) of 50°, or ten ordinary prisms of 60° for each half-prism. The train of two half-prisms has thus a magnifying-power of about 80, and a dispersion or purity of nearly 500°, or 100 ordinary prisms of 60°.

Small though this spectroscope is (the total length being only four inches), its power is extraordinary. With the first half-prism alone the D lines are distinctly separated, whilst with the two half-prisms their angular distance is (by actual measurement) $\frac{2}{3}$°; and this separation, be it remembered, is obtained solely by the prisms, without any magnifying-power from an eyepiece. This separation is about equal to that in the large Greenwich spectroscope with its ten compound prisms of 1·6 inch section and the ordinary eyepiece, whilst the definition is fully as sharp and the brightness far superior. In fact where it is not necessary to magnify the breadth of the spectrum, the miniature spectroscope would appear to be quite equal to the ordinary form, as there is no use in increasing the height of the prisms beyond what is required to fill the pupil of the eye with light. Thus in the miniature spectroscope the prisms need be only 0·2 inch high; whilst if a magnifying-power of 10 is to be used (in the direction of the breadth of the spectrum) their height should be 2 inches to give the same brightness. Except in the case of stars and other objects of very limited apparent size, breadth may be given to the spectrum just as well by increasing the height of the slit as by increasing the height of the prisms. Of course the breadth of the first half-prism should be as large as may be, the magnifying-power in this direction being in all cases high enough to reduce the breadth of the pencil to a small fraction of the diameter of the pupil of the eye.

The results obtained when the train of prisms is reversed so as to give great purity are even more remarkable. With either of the half-prism trains in this position, without slit, collimator, or telescope, the sodium lines are seen with great brilliancy (though not divided) in the flame even of an ordinary paraffin or composite candle without any salt on the wick, and the Fraunhofer lines are seen sharply defined when the train of prisms pure and simple is pointed to the sun or moon. When the two trains are combined as in (5), page 29, still without any slit or lenses, the sodium-lines in an ordinary flame are widely separated, forming two images of the flame; and the *d* and *b* lines in the sun are also well divided. From the low altitude of the sun and the prevalence of haze, I have not had a fair opportunity of trying whether the prominences could be seen in this way without any slit or lenses; but from the ease with which the C line is seen, I think it probable that the prominences would be visible as a ring of light on the background of the spectrum formed by the successive coloured images of the sun's disk, dazzling though it is. Even if this plan should fail with these short trains of prisms, there would be no difficulty in adding one or two more prisms to each; for I imagine that the limit to the power is far from having been reached in the spectroscopes I have described. I have deferred making this new plan of spectroscope known until I could try it in a tolerably complete form, although the first experiments I made with direct-vision and other half-prisms in October last convinced me of its practical value. The results which have been obtained so far will, I hope, show that there is no difficulty in applying the method so as to obtain in a compact form and at a small cost a dispersive power exceeding that of any spectroscope on the old plan, and free from the defects inherent in a complicated instrument.

In conclusion, I may allude to an application of the remarkable property of the half-prism which may perhaps prove of practical use. From what precedes it will be clear that the half-prism, as far as its magnifying-power is concerned, is equivalent to a combination of an object-glass and cylindrical eyepiece, the peculiarity being that it magnifies the angle between two pencils of parallel rays without affecting the parallelism of the rays in each pencil. The half-prisms which have so far been considered are constructed so as to give great dispersion, and the sun is seen through them as if through a telescope with an object-glass formed of a convex flint lens and a concave crown. But it is obvious that an achromatic prism may be formed on the same principle as the achromatic object-glass, giving cylindrical magnifying-power without dispersion. If two such prisms be crossed at right angles, one behind the other, the magnifying-power will be the same in both directions, and the combination will act as an achromatic telescope with the advantage of great compactness, since the eye can be applied close to the second prism. Whether, however, this advantage would compensate for the greater quantity of glass required and for the loss of light is a matter for practical consideration; and I therefore defer the discussion of the achromatic half-prism till I have satisfied myself that it has some practical utility, even though it may never replace the refracting telescope for general use.

Royal Observatory, Greenwich,
1877, Jan. 17.

March 8, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Magneto-electric Induction in Liquids and Gases.—Part I. Production of Induced Currents in Electrolytes." By J. A. Fleming, B.Sc. (Lond.). Communicated by Prof. Stokes, Sec.R.S. Received February 6, 1877.

(Abstract.)

This paper contains an account of an experimental inquiry into the production of induced currents in liquids by magneto-electric induction. Faraday examined one such case of induction, in which a conducting

liquid was used as a secondary circuit. He coiled round the armature of an electromagnet an india-rubber tube filled with dilute sulphuric acid, and found, on making and breaking the primary circuit, the induced currents generated in it, as in the case of metallic conductors; but he could not obtain any effect when brine, sulphuric acid, or other solutions were rotated in basins over a magnet, or enclosed in tubes and passed between the poles. He failed also to detect any magneto-electric current in water flowing across the earth's lines of magnetic force (viz. in the river Thames).

Since the reason for these negative results is not at once obvious, it seemed desirable to repeat and extend them to other cases, so that, if possible, the analogy of electrolytic with solid conductors might, in respect to magneto-electric induction, be completed. In addition, the subject involves the interesting question of the magneto-electric phenomena accompanying the flow of ocean-currents and other large masses of water.

Three cases of induction in liquids flowing in a magnetic field or traversed by lines of magnetic force have been examined.

1. *Production of induced current in a liquid stream flowing uniformly in a constant magnetic field.*—When a stream of conducting fluid flows vertically down between the poles of a magnet a transverse current is produced in a direction at right angles to the lines of force and line of flow. This was obtained in the following way:—A glass tube, about 200 centims. long and 2 centims. wide, had platinum plates 15 millims. wide placed along its inside and at opposite sides, with their lengths parallel to the axis of the tube. Platinum wires welded to these plates were sealed through the glass. The plates were curved to lie closely against the sides of the tube. This tube was placed vertically between the poles of a large electromagnet, the line joining the platinum plates being at right angles to the line of the poles.

To the upper end of the tube was attached another, leading to a reservoir of dilute sulphuric acid placed high above the floor; to the lower end a tube leading to a receptacle on the floor. The platinum plates were then connected with a distant galvanometer. When the magnet was not excited, no flowing of the liquid had any effect on the galvanometer; but when it was excited, at the moment the flow began the galvanometer showed a deflection of 10° to 15°. Since the only part of the galvanometer circuit in motion is the liquid, this deflection was due to the magneto-electric current generated in it by its movement. It was noticed that the plates were *polarized* by the currents so created. As a consequence of this, the deflection of the needle soon fell to zero; and on the liquid flow being stopped, a polarization current in the opposite direction was obtained. This proved that in experiments on induction in liquids, in order to obtain any constant current, non-polarizable electrodes must be used.

2. *Production of induced current in a mass of liquid rotating over a magnetic pole.*—In this case radial currents should be produced. They were obtained as follows:—Flat porous cells were placed round the circumference of a large basin, and in the centre a cylindrical one. These were filled with a solution of cupric sulphate, and contained copper plates. The basin was filled with dilute sulphuric acid. The centre copper plate was connected with one pole of the galvanometer, and the circumferential ones with the other. The whole was placed over the pole of the electromagnet. On exciting the magnet and rotating the dilute acid, a constant current was obtained, flowing from centre to circumference or the reverse according to the direction of rotation. With platinum electrodes the effect cannot be obtained, but with non-polarizable electrodes it is easily produced. Mercury was likewise tried with still better results.

3. *Production of induced current in a liquid at rest in a variable magnetic field.*—If a flexible tube filled with conducting liquid is wound round an electromagnet, and into the ends electrodes placed so as to include a galvanometer in the circuit, then induced currents are obtained whenever the strength of the magnet varies. This is the case examined by Faraday. His experiment was repeated by MM. Logeman and Van Breda (Phil. Mag. [IV.] vol. viii. p. 465), who noticed that the electrodes were left polarized after the induced current had passed. These experiments were repeated with more powerful apparatus, using a soft iron wire core within the inducing helix instead of solid iron, and employing a condenser in the primary circuit. Very strong induced currents were obtained, and correspondingly great polarization of the electrodes placed in the ends of the coil of acid. It was hoped that the currents might produce visible inductive electrolysis, but even this improved arrangement did not yield that result. Other saline solutions were tried with similar results.

Lastly, the phenomenon observed by Arago, of the retardation in the vibrations of a magnetic needle oscillated near the surface of liquids, is examined. Evidence is brought forward to show that this is *not*, as in the case of solid plates, due to induced currents created in the liquid—(1) because the retardation is, *cæteris paribus*, not proportional to the conductivity of the liquid but dependent on its volatility; (2) because it takes place equally when a light brass needle, oscillated by torsion, is substituted for the magnet, provided the needle is light and the period of oscillation not very small. Thus a magnetic needle which required 4 min. 20 sec. to suffer a decrement of 25° in the semi-arc of vibration when in air, required over dilute sulphuric acid 3 min. 30 sec., over ether 2 min. 25 sec. A brass needle of the same dimensions exhibited similar effects.

In conclusion the magneto-electric induction taking place in moving masses of water on the earth's surface under the influence of terrestrial magnetism is briefly discussed.

Before the introduction of the absolute system of electro-magnetic measure, there was no means of estimating the electromotive force so brought into play by the flow of a river or ocean stream, and the magnitude of the effect was perhaps overestimated.

A Table is given, showing the electromotive force in volts produced in two or three cases.

	Difference of potential between two sides in volts.
Gulf-stream at lat. 30° N., long. 60° W.	8·6
Equatorial current, lat. 10° N., long. 40° W.	10·0
Dover and Calais tidal current	3·0
Thames at Waterloo Bridge	·016

This electromotive force without doubt generates a current transverse to the direction of the flow; but since the surrounding still water or the river bed or channel is not a non-conductor, any attempt practically to detect it by plates placed on either side of the stream is not likely to succeed, since the current through the galvanometer is only a derived portion of the current in the stream.

A comparison of a chart of ocean-currents with one of the isogonic lines does not seem to show any distortion of the lines of equal variation where they cut across. If, now, electric currents of any great magnitude were generated in ocean-currents, such would undoubtedly be the case. Though Faraday's failure to detect any magneto-electric current in the Thames may have been partly due to his employment of polarizable electrodes, still there is evidence enough to show that these currents, though certainly existing and capable of being produced on a laboratory scale, cannot be regarded as contributing in any sensible degree towards affecting the form and distribution of the isogonic lines. Those who have looked to this as a possible partial cause of the irregularity observed have been led, no doubt, by the dimensions of the streams to exaggerate the magneto-electric induction caused by their flow.

II. "On the Structure and Development of Vascular Dentine." By Charles S. Tomes, M.A. Communicated by John Tomes, F.R.S. Received February 6, 1877.

(Abstract.)

The nomenclature and classification of the varieties of dentine have hitherto been based solely upon the appearances discoverable in dried teeth; in the present communication the author seeks to amend and place upon a more satisfactory basis the grouping of these several kinds of dentine, by bringing to bear upon their arrangement observations upon the nature of the contents of those large tubes which give to the tissues their name of "vascular" dentine, and, more especially, observations upon the methods by which they are developed.

VASO-DENTINE is the term generally used to designate a variety of dentine exceedingly common in the class of Fish, in which the substance of the tooth is permeated by a number of anastomosing tubes, of considerable size, which have been called "medullary" canals, as they have been supposed to contain pulp-tissue; whilst OSTEO-DENTINE is used to designate that variety of vaso-dentine in which the matrix is arranged in concentric layers round the canals, like the laminæ of an Haversian system in bone, and in which spaces like the lacunæ of bone occur.

The author would not propose to introduce any new terms, but to render more precise and definite the meaning attached to the terms vaso-dentine and osteo-dentine, premising that the application of the two words will be greatly altered by so doing.

The author defines VASO-DENTINE as a modification of dentine which is permeated by a system of canals far larger than ordinary dentinal tubes, which anastomose freely with one another, and contain capillary blood-vessels and nothing else. That is to say, each several canal contains a capillary of the same calibre as itself, and no cellular or other pulp-tissue, for which, in fact, there is no room; the canals were formed by the enclosure of capillaries of the pulp in a calcified matrix. True dentinal tubes may coexist with the large capillary canals; but if they do, they radiate from the central pulp-chamber and not from the canals: in the most typical vaso-dentine, such as that of the hake, the matrix is solid and there are no true dentinal tubes. Vaso-dentine is developed from a sharply defined "membrana eboris," or layer of odontoblast cells.

OSTEO-DENTINE, on the other hand, is also permeated by a system of large channels, but these do not (except as an accident) contain capillary blood-vessels, nor were they developed around capillaries. True "dentinal tubes" can perhaps hardly be said to exist; but the tubes of small calibre which do exist radiate, not from a common pulp-chamber, but from the several canals.

Its greatest distinction from vaso-dentine lies in the manner of its development. It is not (if we except a thin outer layer of hard dentine with which it is often clothed) developed from a specialized layer of odontoblast cells; but calcifying trabeculæ shoot rapidly from the interior of the first-formed dentine cap through the whole substance of the formative pulp, and the canal-system ultimately formed is due to the partial coalescence of these ossifying trabeculæ leaving interspaces between them. The canals have therefore nothing whatever to do with the blood-vessels of the pulp, and therefore do not correspond very closely with those of vaso-dentine. Osteo-dentine is thus not derived from the calcification of a "membrana eboris," or special layer of odontoblast cells, but by ossification (of cells like osteoblasts) shooting through its whole mass.

Thus the tooth-pulp can be bodily withdrawn from a tooth consisting of vaso-dentine by tearing across the capillaries only, and the interior of the dentine cap will be left smooth; but the pulp can by no possibility be withdrawn from a tooth which is advancing in calcification into osteodentine, because it is permeated through and through by a network of calcifying trabeculæ.

It is possible by careful observation to distinguish in sections of dried teeth true vaso-dentine from osteo-dentine; the majority of teeth consisting of the latter tissue ordinarily pass as consisting of the former (*e. g.* the teeth of the pike, of many Plagiostomi, which really consist of osteodentine, but are always described as vaso-dentine).

The teeth of the hake are selected as an illustration of vaso-dentine; they have large pulps, richly vascular, and red blood circulates abundantly through the capillary channels of the dentine, so that the tooth, when the fish is alive, is brilliantly red.

The matrix of the dentine is dense and solid; *i. e.* it is not permeated by dentinal tubes.

The transition between typical vaso-dentine, such as that of the Gadidæ, and hard unvascular dentine, such as that of most mammalian teeth, is gradual.

Thus most of the Pleuronectidæ have teeth which at their basal halves consist of typical vaso-dentine without dentinal tubes, just like that of the Gadidæ; but above the middle, dentinal tubes radiating out from the central pulp-chamber begin to appear, at first sparsely, and the capillary canals to become fewer, till the apex of the tooth consists of ordinary fine-tubed dentine, in which few, if any, capillary channels exist.

And in *Serrasalmo* there are teeth which are throughout composed of a dentine permeated by dentinal tubes, but in the basal half of the tooth a few capillary channels are present. From such a form of dentine to ordinary hard unvascular dentine is but a short step.

The development of osteo-dentine is illustrated by a description of the teeth of a pike; the outer layer is developed, like dentine, from a layer of cells analogous to, though less specialized than, odontoblasts; and so soon as this has been calcified the interior of the tooth is formed by a rapid ossification, just as the subjacent bone is formed.

Vaso-dentine therefore differs much less from true or unvascular dentine than osteo-dentine does, the relation between the three tissues being well seen in the teeth of Sparidæ.

In *Sargus ovis* the incisor-like front teeth appear to be implanted by long roots; these are formed by the dentinal formative pulps, just as are the roots of ordinary rooted teeth.

But there is this peculiarity in the nature of the process: the dentinal pulp, so long as the "crown" (or portion which will be above the bone) is being developed, is converted into fine-tubed *unvascular dentine*; but so soon as the root or implanted portion commences to be formed, this same dentinal pulp, the apex of which is even yet forming unvascular dentine, calcifies into *vaso-dentine.* Without there being any exact break or breach of continuity, the change from true dentine to vaso-dentine is sudden, and the tooth is easily broken off at this point. When the greater part of the length of the root has been formed the manner of calcification again changes, this time not so abruptly, till near to the end of the root the dentinal pulp becomes converted into *osteo-dentine*, which is quite indistinguishable from and blends insensibly with the surrounding coarse bone by which the tooth is fastened into the socket; there is, in fact, no reason for calling it any thing else than coarse bone, except the fact that it is the product of calcification of a dentinal pulp. In this case a single dentinal pulp forms first hard dentine, secondly vaso-dentine, and at last osteo-dentine.

Another variety of complex dentine is brought about by foldings and subdivisions of the formative pulp: both vaso-dentine and osteo-dentine are formed by the calcification of simple pulps; but in many instances the odontoblast-bearing surface of the pulp is itself complicated in form, and a dentine arranged as it were round many pulp-chambers is the result.

For this no better name than PLICI-DENTINE (also a term already in use) suggests itself: it is to be seen in its simpler form at the base of the teeth of *Lepidosteus*, in greater complexity at the base of the teeth of *Varanus*, and in exceeding complexity in the teeth of Labyrinthodonts.

The author would distinguish, therefore:—

(i) *Hard unvascular dentine*, the characters of which are sufficiently known.

(ii) *Vaso-dentine*, which is developed from odontoblasts after the manner of dentine, but contains an anastomosing network of canals modelled around and containing capillaries.

(iii) *Plici-dentine*, developed from odontoblasts, but from a complicated pulp, so that it is more or less divided up into distinct systems of dentinal tubes.

(iv) *Osteo-dentine*, developed from osteoblasts, like bone, and quite unlike dentine; permeated by a system of large canals, which do not contain, or have any special relation to, blood-vessels.

The author lays no stress on the characters formerly given as distinctive of osteo-dentine (*i. e.* a laminated arrangement of the matrix and the presence of lacunæ), because (i) lamination of the matrix is not unknown in vaso-dentine, (ii) lacunæ are very frequently absent from bone in fishes, and very frequently from osteo-dentine, so that these characters, as those who have tried to apply them have found, are not useful in practice.

The attachment of the teeth of the hake is so peculiar as to merit a word of notice: the inner and longer of the two rows of teeth are set upon elastic hinges, which allow of their being bent inwards towards the throat, but cause them at once to spring back into the upright position when pressure is taken off them. This arrangement, shared by the angler, was hardly to be expected in one of the Gadidæ; but the author has found in others of the family steps towards this highly specialized arrangement, the benefit of which to a voracious predatory fish, such as the hake, is obvious*.

The common cod has teeth which admit of a small amount of motion only; but a comparison of them with those of the hake shows clearly that a further modification in the same direction would lead to an attachment similar to that of the latter fish.

The haddock, which in this respect is a fair representative of the family, has teeth which admit of no motion at all.

III. "Note on the Early Stages of Development of the Nerves in the Chick." By A. MILNES MARSHALL, B.A., B.Sc., of St. John's College, Cambridge. Communicated by Dr. MICHAEL FOSTER, F.R.S., Prelector of Physiology in Trinity College, Cambridge. Received February 13, 1877.

In the investigations here described embryos of ages from thirty-six hours to four days were employed. These were, for the most part, hardened by immersion in picric acid, prepared after Kleinenberg's method, for three to five hours, and then in alcohol of gradually increasing strength. It is to the use of picric acid as a hardening agent that the results obtained are believed to be in large measure due. All the more important results have, however, been confirmed by specimens hardened in chromic acid in the usual manner, though such specimens have almost invariably proved inferior in distinctness to those prepared with picric acid. Good results have also been obtained from duck-embryos hardened in picric acid.

Owing to the less compact character of the mesoblast of the head and to the absence of protovertebræ, the development of the cranial nerves is easier to study than that of the spinal, and will therefore be considered first.

Transverse sections through the hind brain of a forty-three hours' chick show that the cells along the median dorsal line are more spherical in shape and slightly smaller than those composing the rest of the brain; also that these spherical cells grow upwards, so as to form a conspicuous longitudinal ridge running along the upper surface of the hind brain immediately beneath the external epiblast.

This ridge is traceable along the whole length of the hind brain, but is much more prominent posteriorly than it is in front, where it gradually disappears. At intervals the ridge becomes more prominent, and grows out laterally into paired processes. These processes are the rudiments of the cranial nerves; the cells composing them are, like those of the ridge itself, small and spherical, and differ markedly from both the elongated cells of the external epiblast, and the large, loosely arranged, branching and irregularly shaped mesoblast cells.

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* Since the accompanying paper was placed in the hands of the Royal Society, the author has found that the vomerine and palatine teeth of the pike present a similar hinged manner of attachment, so that they oppose no obstacle to the swallowing of prey seized by the fish.

At forty-three hours the first pair of these processes arises from the anterior part of the hind brain; it subsequently develops into the fifth nerve.

Immediately in front of the auditory involution (which at this period is a wide and very shallow pit) a large outgrowth arises on either side, from which the facial and auditory nerves are derived.

A large outgrowth from the median ridge commences on either side a short distance behind the auditory pit, and is of considerable longitudinal extent, reaching as far back as the middle of the first protovertebra. From this outgrowth are developed the glossopharyngeal nerve and the several branches of the vagus.

The outgrowth of spherical cells from the summit of the neural canal, forming the longitudinal ridge above alluded to, is not confined to the hind brain, but is continued backwards without any break some distance down the spinal cord. In the spinal cord, as in the brain, this ridge gives off at intervals paired lateral processes, which extend outwards just beneath the superficial epiblast. These processes correspond in number to the protovertebræ, and are the rudiments of the posterior roots of the spinal nerves. Each process has a longitudinal extension equal to about half a protovertebra, opposite the posterior part of which it is situated. In the case of the first few spinal nerves the processes are somewhat larger, and extend back so as to overlap the anterior parts of the succeeding protovertebræ.

This description, it is believed, differs from any previously published account of the development of the nerves in the chick, but agrees remarkably closely with Balfour's* account of the development of the nerves, both cranial and spinal, of Elasmobranchs, and is in accordance with Hensen's† observations on the development of the posterior roots of the spinal nerves in the rabbit.

Opposite the centre of each protovertebra the external epiblast grows downwards as a small conical process on either side of the spinal cord and in close contact with it. These processes were mistaken by His‡ for the commencements of the spinal nerves, but are clearly seen to have no connexion whatever with the nerve-rudiments. His is the only previous observer who assigns an epiblastic, instead of a mesoblastic, origin to the nerves in the chick; he, however, derives them directly from the external epiblast, while, according to the description just given, they really arise from the involuted epiblast of the neural canal.

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* Phil. Trans. vol. 166, pt. 1.

† Zeitschrift f. Anatomie u. Entwickelungsgeschichte, 1876, Bd. i.

‡ Die erste Anlage des Wirbelthierleibes.

From their mode of origin the cranial and the anterior spinal nerves will be seen to be all connected together at first by a longitudinal commissure of spherical cells, while the two nerves of each pair, whether cranial or spinal, are also connected together across the top of the neural canal.

The attachment of the nerve, whether cranial or spinal, is at first to the extreme summit of the neural canal. Shortly after their appearance the attachments shift slightly outwards, and, in the case of the spinal nerve, become much more slender. This shifting is believed to be apparent rather than real, and to be caused, as first suggested by Balfour, by rapid growth of the cells at the summit of the canal, which has the effect of separating the roots of the two sides from one another and forçing them apart.

Though the proximal part of the nerve-root becomes thus more slender in the spinal nerves, the distal part enlarges considerably, and grows down as an oval mass (the spinal ganglion) between the spinal cord and the protovertebræ. At this period the most prominent part is situated opposite the interval between two protovertebræ.

During the third day a great change occurs in the point of attachment, which is now considerably lower down, in the position occupied by the root in the adult. The nerve is now attached, not by its apex, but by a small process growing out from its side, and projects considerably above the point of attachment. Owing to the surrounding mesoblast this stage is very difficult to investigate; but the appearances strongly suggest that the original attachment of the nerve to the summit of the cord is lost, and a new one acquired lower down, and that the projection of the nerve above the point of attachment, which becomes inconspicuous very shortly afterwards, is a remnant of the original attachment.

The anterior roots of the spinal nerves arise later than the posterior, and have not been observed earlier than the latter part of the third day. They appear as small outgrowths from the lower part of the sides of the spinal cord, and from the first occupy the position held by them in the adult. This position is indicated before the actual appearance of the roots by a slight convergence of the cells at the outer part of the cord. The anterior roots are very slender, and consist of much elongated cells, contrasting strongly with the spherical or oval cells of the posterior roots.

Early on the fourth day each anterior root consists of a number of such processes placed one behind the other, and lying opposite the anterior half of a protovertebra. The total length of attachment of an anterior root on the fourth day is equal to about half a protovertebra.

The anterior roots grow outwards, and early in the fourth day join with the posterior roots to constitute the spinal nerves.

In the cranial nerves no anterior roots have been observed; but as the observations have not been carried beyond the fourth day, and certain of the cranial nerves have not been observed at all, no conclusion as to their non-existence is to be drawn from this fact, which can only be considered a doubtful confirmation of Balfour's failure to discover anterior cranial roots in Elasmobranchs *.

The facial and auditory nerves have been seen to arise as a single outgrowth just in front of the ear; this speedily divides into an anterior part, which runs downwards in front of the auditory vesicle and becomes the facial nerve, and a posterior part, which is closely applied to the anterior wall of the auditory vesicle and becomes the auditory nerve.

The fifth nerve arises as a single outgrowth on either side, the position of which is very constant. The so-called "hind brain" consists at forty-three hours of an apparently variable number of dilatations separated by slight constrictions, and gradually decreasing in size from before backwards. These dilatations are well known, but appear to possess more constancy than is usually ascribed to them; the most anterior of them is but little smaller than the mid brain. From it the fifth nerve arises in all the specimens examined.

The third, fourth, and sixth nerves have not been observed; but a slight outgrowth from the summit of the mid brain, noticed in two specimens only, may prove to be the commencement of the third or fourth.

The olfactory nerves arise towards the end of the third day as solid outgrowths from the anterior end of the fore brain, close to the median dorsal line, and exactly correspond in mode of development and in appearance with the other cranial nerves and with the posterior roots of the spinal nerves. They arise at a time when a section through the anterior part of the fore brain transverse to its longitudinal axis, and passing through the olfactory pits and nerves, is almost perfectly circular in outline, and must therefore be described as arising from the fore brain itself, and not from the cerebral hemispheres, with which they have no connexion at first, and which are not nearly such prominent objects at the end of the third day as they are often described to be. There is no trace of an "olfactory vesicle" in the early stages.

This mode of development of the olfactory nerve in the chick would seem to be of considerable morphological importance, since, if confirmed, any arguments concerning the composition of the skull, based on the distribution of the cranial nerves, would have in future to take the olfactory nerves into consideration.

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* *Loc. cit.* p. 189

IV. “Notes on Physical Geology.” By the Rev. SAMUEL HAUGHTON, M.D. Dublin, D.C.L. Oxon., F.R.S., Professor of Geology in the University of Dublin. Received February 15, 1877.

No. I. *Preliminary Formulæ relating to the internal change of position of the Earth's Axis, arising from Elevations and Depressions caused by Geological Changes.*

1. If the earth's surface be an ellipsoid of revolution, whose moments of inertia round the polar and equatorial axes are C and A, and if μ be the mass of a mountain placed on any meridian, with coordinates z, x, it is required to find the change of position in the earth's axis caused by the addition of the mass μ (supposed in the first instance to be placed upon the earth *ab extra*). If λ be the latitude on which μ is placed, we have $\tan\lambda=\frac{z}{x}$; and if θ be the angle made with the earth's axis by any axis in the meridian of μ, if I be the total moment of inertia round this axis, we have

$$\begin{aligned}\mathrm{I}=&\,\mathrm{A}\sin^2\theta+\mathrm{C}\cos^2\theta\\&+\mu\,(x^2\cos^2\theta+z^2\sin^2\theta-2\,xz\sin\theta\cos\theta)\,.\quad.\quad.\quad.\quad(1)\end{aligned}$$

The new axis of rotation is that which makes

$$\mathrm{I}=\text{maximum, or } d\mathrm{I}=0,$$

from which we find, after some reduction,

$$-\tan 2\theta=\frac{2\mu xz}{(\mathrm{C}-\mathrm{A})+\mu(x^2-z^2)}\,.\quad.\quad.\quad.\quad.\quad(2)$$

If we make $\theta=$ maximum, or $d\theta=0$, we ascertain the position in which the mass μ must be placed so as to produce the maximum shift in the position of the earth's axis.

Differentiating (2), we find

$$x\{(\mathrm{C}-\mathrm{A})+\mu(x^2+z^2)\}dz+z\{(\mathrm{C}-\mathrm{A})-\mu(x^2+z^2)\}dx=0\,;$$

and from the equation of the ellipse

$$\frac{z^2}{c^2}+\frac{x^2}{a^2}=1$$

we have

$$\frac{zdz}{c^2}+\frac{xdx}{a^2}\,;$$

from which we obtain, finally,

$$(C-A)\left(\frac{z^2}{c^2}-\frac{x^2}{a^2}\right)-\mu(z^2+x^2)=0;$$

or, since $\frac{z}{x}=\tan\lambda$, after some reductions,

$$\tan^2\lambda=\frac{c^2}{a^2}\frac{(C-A)+\mu a^2}{(C-A)-\mu c^2}. \quad \dots\dots\dots\dots \quad (3)$$

This determines the position in which the mass μ will produce the maximum effect in displacing the earth's axis.

2. In order to make use of the preceding formulæ in calculation, it is necessary to determine the absolute numerical values of C and A, which may be done as follows:—

From Clairaut's theorem* we obtain

$$C-A=\frac{Ma^2}{3}(2\epsilon-q), \quad \dots\dots\dots\dots \quad (4)$$

where M=mass of the earth,

a=equatorial radius,

$\epsilon=\text{ellipticity}=\frac{1}{300}$,

q=ratio of centrifugal force to gravity at the equator $=\frac{1}{289}$.

From observations on Precession and Nutation† we find

$$\frac{C-A}{C}=\frac{1}{306}. \quad \dots\dots\dots\dots \quad (5)$$

From equations (4) and (5) we obtain

$$\left.\begin{aligned} C&=\frac{Ma^2}{3\cdot06}, \\ A&=\frac{Ma^2}{3\cdot07}, \\ C-A&=\frac{Ma^2}{935\cdot6}. \end{aligned}\right\} \quad \dots\dots\dots\dots \quad (6)$$

Substituting in (2) and assuming $\rho=\frac{\mu}{M}$, we find

$$-\tan 2\theta=\frac{935\cdot6\rho\sin 2\lambda}{1+935\cdot6\rho\cos 2\lambda};$$

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* Cambridge and Dublin Mathematical Journal (new series), vol. vi. p. 184.

† Leverrier and Serret, 'Annales de l'Observatoire de Paris,' 1859, p. 324.

or, neglecting small quantities,

$$-\tan 2\theta = 935{\cdot}6\, \rho \sin 2\lambda. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

This equation shows that the pole moves away from the mass μ, and that this mass is most effective at the latitude of 45°.

3. In order to apply the preceding to the case of our actual continents and oceans, we integrate (7) along the meridian as follows,

$$\rho = \frac{\mu}{M} = \frac{r^2 tdl}{M} . \cos \lambda d\lambda,$$

where

r = radius of earth,
l = longitude,
λ = latitude,
t = height of continent or depth of sea above or below the zero plane.

Hence we have

$$\tan 2\theta = 2\theta = -935{\cdot}6\, \frac{r^2 tdl}{M} \int_{\lambda}^{0} \cos \lambda \sin 2\lambda d\lambda,$$

and finally

$$\theta = -935{\cdot}6\, \frac{r^2 tdl}{M} (1 - \cos^3 \lambda). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The zero plane, from which t is measured, is the surface of the ellipsoid similar to the sea-surface, and containing the same volume as the total solid matter of the globe. It is thus found: assuming the mean height of the continents above the sea-level at about 1000 feet, and the mean depth of the ocean at about two miles, we have, in miles,

$$x = \frac{2 . 2L}{W + L} . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where x is the height of the zero plane above the present mean sea-bottom, and L, W are the areas of land and water:

L = 52 millions of square miles.
W = 145 „ „ „

Substituting in (9) we find

$$x = 0{\cdot}58 \text{ mile}.$$

The zero plane, therefore, or original surface of the solid earth before it became wrinkled by geological forces, lies at a depth of 1·42 foot below the sea-level. In using equation (8) we must therefore write

$t = +1{\cdot}62$ mile (continent).
$t = -0{\cdot}58$ „ (ocean).

In calculating the motion of the pole caused by the ocean excavations, the weight of the sea-water must be considered, and, by chance, it happens that the weight of the sea-water somewhat more than counterbalances the weight of the surface-rock excavated; so that the depression of the ocean-surfaces of the earth beneath the zero plane have had little or no effect in shifting the position of the pole.

Assuming 1·026 and 2·75 as the densities of sea-water and surface-rock, we have for the excess of weight of water added above that of rock excavated, expressed in depth of rock, in miles,

$$\frac{2\times 1{\cdot}026-0{\cdot}58\times 2{\cdot}75}{2{\cdot}75}=0{\cdot}17 \text{ mile.}$$

The introduction of the weight of the sea will thus give us (raising the zero plane by 0·17 of a mile)

$$\begin{aligned} t&=+1{\cdot}45 \text{ mile (continent),}\\ t&=0{\cdot}00 \;\;,, \;\;\text{(ocean).}\end{aligned}$$

The formulæ (8) may be brought into a shape fit for calculation in the following way:—multiplying both sides by r we have

$$r\theta=-935{\cdot}6\frac{r^3tdl}{M}(1-\cos^3\lambda),$$

which gives the displacement of the pole in English miles. If we assume, for convenience of the quadrature,

$dl=5°$,

we have $2r=7916$ miles,

$rdl=345$,,

$t=1{\cdot}45$ mile,

$M=\frac{8}{3}\varpi r^3$ cubic miles of surface-rock, which has half the mean density of the entire earth.

We may calculate from these data

$M=519440$ million cubic miles of surface-rock,

$r^3tdl=7836{\cdot}6$,, ,, ,,

Hence, finally,

$$r\theta=-14{\cdot}11(1-\cos^3\lambda). \quad \dots\dots\dots \quad (10)$$

This equation expresses that a semilune of continent 5° in width, elevated from the pole to the equator, being 345 miles in width at the equator and zero at the pole, will push the earth's axis away from it though a distance of 14·11 miles.

If we imagine a continent occupying 90° of longitude of a semilune, and extending from the equator to the pole, we find, if l denote the hour-angle from the meridian bisecting the continent,—

Long.	$1-\cos^3\lambda$.	$2\cos l\,(1-\cos^3\lambda)$.
45°	1·00	1·42
40	1·00	1·52
35	1·00	1·64
30	1·00	1·74
25	1·00	1·80
20	1·00	1·88
15	1·00	1·92
10	1·00	1·96
5	1·00	1·98
0	1·00	2·00
	Total....	17·86

The displacement of the pole, in miles, produced by this imaginary continent is, by equation (10),

$$r\theta = -14{\cdot}11 \times 17{\cdot}86 = -252 \text{ miles.}$$

No. II. *On the amount of shifting of the Earth's Axis, already caused by the elevation of the existing Continents.*

Having shown in the preceding note that the motion of the earth's axis caused by the geological wrinkling of the earth's surface depends (in consequence of the weight of the sea-water) only on the continents, it remains for me to calculate the numerical amount of change of axis produced by each of the existing continents.

For this purpose I select the following meridians for the coordinates Y and X of the motion :—

Greenwich	0°	+Y
Rangoon	90	−X
Behring's Strait	180	−Y
Yucatan	270	+X

Reckoning the longitudes eastward, round the whole circumference of the earth, the equation (10) generalized becomes

$$r\theta = -14{\cdot}11\,(\cos^3\lambda' - \cos^3\lambda), \quad . \quad . \quad . \quad . \quad . \quad (11)$$

in which the meridian of each 5° of longitude is used, λ' and λ being the lowest and highest degrees of latitude of the land on each meridian.

The expression $\cos^3\lambda' - \cos^3\lambda$ is found by observation on the globe, and resolved into its components X and Y, regarding the North Pole as the axis moved. We thus find

I. EUROPE AND ASIA.

Long.	$\cos^3 \lambda' - \cos \lambda$.	X.	Y.	
350°	0·18	−0·03	−0·17	
355	0·35	−0·03	−0·34	
360	0·18	−0·00	−0·18	X = −0·06
5	0·23	+0·02	−0·23	
10	0·28	+0·05	−0·27	
15	0·28	+0·07	−0·27	
20	0·39	+0·13	−0·36	
25	0·49	+0·20	−0·44	
30	0·49	+0·24	−0·42	
35	0·76	+0·43	−0·62	
40	0·83	+0·53	−0·63	
45	0·84	+0·59	−0·59	
50	0·78	+0·60	−0·50	
55	0·76	+0·62	−0·43	
60	0·68	+0·59	−0·34	
65	0·68	+0·62	−0·29	
70	0·84	+0·79	−0·29	
75	0·89	+0·86	−0·23	
80	0·77	+0·76	−0·13	
85	0·68	+0·67	−0·05	
90	0·77	+0·77	0·00	Y = −6·78
95	0·89	+0·88	+0·08	
100	0·89	+0·87	+0·15	
105	0·89	+0·86	+0·23	
110	0·77	+0·72	+0·26	
115	0·77	+0·70	+0·32	
120	0·49	+0·42	+0·29	
125	0·49	+0·40	+0·28	
130	0·39	+0·30	+0·26	
135	0·39	+0·27	+0·27	
140	0·13	+0·08	+0·10	
145	0·06	+0·03	+0·05	
150	0·06	+0·03	+0·05	
155	0·11	+0·05	+0·10	
160	0·06	+0·02	+0·05	
165	0·06	+0·01	+0·06	
170	0·06	+0·01	+0·06	
175	0·01	0·00	+0·01	
180	0·00	0·00	0·00	
		X = +14·19	Y = +2·62	

Hence we obtain, finally,

$$X = -0{\cdot}06 + 14{\cdot}19 = +14{\cdot}13,$$
$$Y = -6{\cdot}78 + \ 2{\cdot}62 = -4{\cdot}16.$$

Multiplying these results by 14·11, the coefficient of equation (11), we ind the following displacements in miles :—

X (towards Yucatan) = 199·4 miles,
Y (towards Behring's Strait) = 58·7 miles.

Compounding these together we find

$$\sqrt{X^2+Y^2} = 207{\cdot}1 \text{ miles},$$
$$\frac{X}{Y} = \tan\phi,$$
$$\phi = 73^\circ\ 35' \text{ W. of Greenwich.}$$

This resultant coincides with the meridian of the Andes.

II. Africa.

North Africa.

Long.	$\cos^3\lambda - \cos^3\lambda$.	X.	Y.	
345°	0·21	−0·05	−0·20	
350	0·44	−0·07	−0·43	
355	0·44	−0·04	−0·43	X = −0·16
360	0·44	0·00	−0·44	
5	0·44	+0·04	−0·43	
10	0·32	+0·05	−0·31	
15	0·35	+0·09	−0·34	
20	0·35	+0·12	−0·33	
25	0·35	+0·15	−0·32	
30	0·35	+0·17	−0·30	
35	0·17	+0·10	−0·14	
40	0·05	+0·03	−0·04	X = +0·76
45	0·01	+0·01	−0·01	Y = −3·72

South Africa.

Long.	$\cos^3\lambda - \cos^3\lambda$.	X.	Y.
10	0·17	−0·03	+0·17
15	0·45	−0·11	+0·43
20	0·45	−0·15	+0·38
25	0·35	−0·15	+0·32
30	0·26	−0·13	+0·22
35	0·17	−0·10	+0·14
40	0·00	0·00	0·00
45	0·21	−0·15	+0·15
		X = −0·82	Y = +1·81

Adding all together we obtain, finally,

X = −0·22 = 3·1 miles (towards Rangoon),
Y = −1·91 = 26·9 miles (towards Behring's Strait).

III. North America.

Long.	$\cos^2\lambda' - \cos^2\lambda$.	X.	Y.	
195°	0·06	−0·01	+0·06	
200	0·13	−0·04	+0·12	
205	0·06	−0·02	+0·05	
210	0·06	−0·03	+0·05	
215	0·06	−0·03	+0·05	
220	0·06	−0·04	+0·04	
225	0·13	−0·09	+0·09	
230	0·21	−0·16	+0·13	
235	0·49	−0·40	+0·28	
240	0·49	−0·42	+0·21	
245	0·68	−0·61	+0·28	
250	0·68	−0·64	+0·23	
255	0·77	−0·74	+0·20	
260	0·84	−0·83	+0·14	
265	0·68	−0·61	+0·06	
270	0·65	−0·65	0·00	Y = +1·99
275	0·46	−0·45	−0·04	
280	0·59	−0·58	−0·10	
285	0·38	−0·37	−0·10	
290	0·23	−0·19	−0·08	
295	0·08	−0·07	−0·04	
300	0·14	−0·12	−0·07	
305	0·07	−0·06	−0·04	
310	0·12	−0·09	−0·08	
315	0·12	−0·08	−0·08	
320	0·07	−0·04	−0·05	
325	0·07	−0·04	−0·06	
330	0·06	−0·03	−0·05	
335	0·11	−0·04	−0·10	
340	0·02	0·00	−0·02	
		X = −7·48	Y = −0·91	

Hence, finally,

$X = \qquad -7{\cdot}48,$
$Y = +1{\cdot}99 - 0{\cdot}91 = +1{\cdot}08\,;$
$X = -7{\cdot}48 = 105{\cdot}5$ miles (towards Rangoon),
$Y = +1{\cdot}08 =$ 15·2 miles (towards Greenwich).

IV. South America.

North of Equator.

Long.	$\cos^3\lambda' - \cos^3\lambda$.	X.	Y.
280°	0·05	−0·04	−0·01
285	0·05	−0·04	−0·01
290	0·05	−0·04	−0·02
295	0·05	−0·04	−0·02
300	0·01	−0·01	0·00
305	0·01	−0·01	0·00
		X=−0·18	Y=−0·06

South of Equator.

Long.	$\cos^3\lambda' - \cos^3\lambda$.	X.	Y.
280	0·05	+0·05	+0·01
285	0·10	+0·09	+0·02
290	0·88	+0·82	+0·30
295	0·55	+0·50	+0·23
300	0·47	+0·41	+0·24
305	0·45	+0·37	+0·26
310	0·26	+0·20	+0·17
315	0·25	+0·17	+0·17
320	0·09	+0·06	+0·07
		X=+2·67	Y=+1·47

Hence, finally,

$X = -0{\cdot}18 + 2{\cdot}67 = +2{\cdot}49,$
$Y = -0{\cdot}06 + 1{\cdot}47 = +1{\cdot}41\,;$
$X = +2{\cdot}49 = 35{\cdot}1$ miles (towards Yucatan),
$Y = +1{\cdot}41 = 19{\cdot}9$ miles (towards Greenwich).

V. AUSTRALIA AND PACIFIC ISLANDS.

Islands.

Long.	$\cos^2\lambda' - \cos^2\lambda$.	X.	Y.
100°	0·01	−0·01	0·00
105	0·01	−0·01	0·00
110	0·16	−0·15	−0·05
115	0·33	−0·30	−0·14
120	0·29	−0·25	−0·15
125	0·35	−0·28	−0·20
130	0·25	−0·19	−0·16
135	0·29	−0·20	−0·20
140	0·54	−0·35	−0·41
145	0·48	−0·27	−0·39
150	0·10	−0·05	−0·08
155	0·00	0·00	0·00
160	0·00	0·00	0·00
165	0·14	−0·04	−0·13
170	0·20	−0·03	−0·19
175	0·05	−0·01	−0·04
		X = −2·14	Y = −2·14

X = −2·14 = 30·2 miles (towards Rangoon),
Y = −2·14 = 30·2 miles (towards Behring's Strait).

Collecting all the preceding results into one Table, we see the relative effects of the elevation of each of the existing continents upon the position of the pole.

Displacement of North Pole caused by each continent.

	Towards Greenwich. miles.	Towards Behring's Strait. miles.	Towards Yucatan. miles.	Towards Rangoon. miles.
Europe and Asia	——	58·7	199·4	——
Africa	....	26·9	——	3·1
North America	15·2	——	——	105·5
South America	19·9	——	35·1	——
Australia, &c.	——	30·2	——	30·2

The power of Europe and Asia in moving the pole is partly due to the extension of this continent along the parallel of 45°, which is the most effective latitude. The actual effect produced by Europe and Asia was not much less than that of our imaginary continent (Note I.), occupying one eighth part of the surface of the globe.

The foregoing results are positive, and the motions of the pole indicated must have actually occurred when the existing continents were formed. But simultaneously with these elevations depressions must have gone on elsewhere, continents disappearing beneath the sea and king to the zero plane, while other continents were rising. It is to be noticed that although the excavation of the sea-bottom to its present depth below the zero plane, corrected for the weight of the ocean, produces no motion in the pole, yet that the depression of a continent down to the zero plane produces a motion of pole equal and opposite to that produced by its elevation. I have calculated the hypothetical effects of the depression of imaginary continents occupying the sites of the present Pacific Ocean, with the following results:—

VI. North Pacific Ocean (depressed).

Long.	$\cos^3\lambda' - \cos^3\lambda$.	X.	Y.	
100°	0·10	−0·10	−0·01	
105	0·17	−0·16	−0·04	
110	0·26	−0·24	−0·09	
115	0·26	−0·23	−0·11	
120	0·45	−0·39	−0·22	
125	0·45	−0·38	−0·26	
130	0·73	−0·56	−0·49	
135	0·81	−0·57	−0·57	
140	0·88	−0·56	−0·67	
145	0·88	−0·50	−0·72	
150	0·88	−0·44	−0·76	
155	0·88	−0·37	−0·80	
160	0·88	−0·30	−0·83	
165	0·81	−0·21	−0·78	
170	1·00	−0·17	−0·98	
175	1·00	−0·09	−0·99	
180	1·00	0·00	−1·00	X=−5·27
185	0·97	+0·08	−0·96	
190	0·94	+0·16	−0·92	
195	0·94	+0·24	−0·91	
200	0·94	+0·33	−0·88	
205	0·94	+0·39	−0·85	
210	0·94	+0·47	−0·81	
215	0·90	+0·51	−0·74	
220	0·81	+0·52	−0·62	
225	0·70	+0·49	−0·49	
230	0·70	+0·54	−0·45	
235	0·50	+0·41	−0·29	
240	0·50	+0·43	−0·25	
245	0·26	+0·23	−0·11	
250	0·26	+0·24	−0·09	
255	0·17	+0·16	−0·04	
260	0·10	+0·10	−0·02	
265	0·09	+0·08	−0·01	
270	0·07	+0·07	0·00	
275	0·06	+0·06	0·00	
		X=+5·51	Y=−17·76	

Hence, finally,

$$X = -\ 5{\cdot}27 + 5{\cdot}51 = +0{\cdot}24,$$
$$Y = -17{\cdot}76;$$

or

$$X = +\ 0{\cdot}24 = \ 3{\cdot}4 \text{ miles (towards Yucatan)},$$
$$Y = -17{\cdot}76 = 250{\cdot}6 \text{ miles (towards Behring's Strait)}.$$

This Table shows (*inter alia*) the remarkable symmetry of the North Pacific Ocean east and west of the meridian of Behring's Strait.

VII. SOUTH PACIFIC OCEAN (depressed).

Long.	$\cos^3\lambda' - \cos^3\lambda$.	X.	Y.	
140°	0·45	+0·29	+0·34	
145	0·45	+0·26	+0·37	
150	0·94	+0·47	+0·81	
155	0·94	+0·39	+0·85	
160	0·94	+0·32	+0·88	
165	0·94	+0·24	+0·91	
170	0·94	+0·16	+0·92	
175	0·94	+0·08	+0·94	
180	0·94	0·00	+0·94	X = +2·21
185	0·94	−0·08	+0·93	
190	0·94	−0·16	+0·92	
195	0·94	−0·24	+0·91	
200	0·94	−0·32	+0·88	
205	0·94	−0·39	+0·85	
210	0·94	−0·47	+0·81	
215	0·94	−0·54	+0·77	
220	0·94	−0·60	+0·72	
225	0·94	−0·66	+0·66	
230	0·94	−0·72	+0·60	
235	0·94	−0·77	+0·54	
240	0·94	−0·81	+0·47	
245	0·94	−0·85	+0·39	
250	0·94	−0·88	+0·32	
255	0·94	−0·91	+0·24	
260	0·94	−0·92	+0·16	
265	0·94	−0·93	+0·08	
270	0·94	−0·94	0·00	Y = +17·21
275	0·94	−0·93	−0·08	
280	0·90	−0·88	−0·15	
285	0·40	−0·38	−0·10	
		X = −13·28	Y = −0·33	

Hence, finally,

$$X = +\ 2{\cdot}21 - 13{\cdot}28 = -11{\cdot}07,$$
$$Y = +17{\cdot}21 -\ 0{\cdot}33 = +16{\cdot}88;$$

or
$$X = -11{\cdot}07 = 156{\cdot}2 \text{ miles (towards Rangoon).}$$
$$Y = +16{\cdot}88 = 238{\cdot}2 \text{ miles (towards Greenwich).}$$

The total effect of a continent equal to the North Pacific would be

$$\sqrt{X^2+Y^2} = 250{\cdot}6 \text{ miles},$$
$$\tan(\phi)\frac{X}{Y} = \phi = 0^\circ\ 47' \text{ E. of } 180^\circ.$$

The total effect of a continent equal to the South Pacific Ocean would be

$$\sqrt{X^2+Y^2} = 201{\cdot}8 \text{ miles},$$
$$\tan(\phi)\frac{X}{Y} = \phi = 23^\circ\ 17' \text{ E. of Greenwich.}$$

March 15, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Tides of the Arctic Seas.—Part VII. Tides of Port Kennedy, in Bellot Strait." (Final Discussion.) By the Rev. SAMUEL HAUGHTON, M.D. Dublin, D.C.L. Oxon., F.R.S., Fellow of Trinity College, Dublin. Received February 17, 1877.

(Abstract.)

The tidal observations at Port Kennedy were made hourly for 23 days; and in my former discussion of these tides (Part VI.) I used only the observations made in the neighbourhood of H. W. and L. W., obtaining the following results for the Tidal Coefficients:—

Diurnal Tide.	*Semidiurnal Tide.*
S=23·4 inches.	S=7·0 inches.
$i_s = 5^h\ 12^m$.	$i_s =$
M=20·9 inches.	M=17·0 inches.
$i_m = 0^h\ 34^m$.	$i_m = -0^h\ 12^m$.

In the present discussion I have employed all the hourly observations made during the 23 days, and have obtained the following results:—

Diurnal Tide.	*Semidiurnal Tide.*
$S = 36\cdot4$ inches.	$S = 5\cdot9$ inches.
$i_s = 3^h\ 2^m$.	$i_s = 2^h\ 48^m$.
$M = 18\cdot5$ inches.	$M = 15\cdot5$ inches.
$i_m = -2^h\ 48^m$.	$i_m = 6^h\ 2\frac{1}{2}^m$.

The present more complete discussion fully confirms the result before obtained by me respecting the great magnitude of the Solar Diurnal Tide at this station, and also shows a satisfactory agreement in the other coefficients obtained from H. W. and L. W. observations only.

The method employed in the present paper is based on Fourier's Theorem, by which the height of tide is expressed as follows:—

$$F = A_0 \begin{array}{l} + A_1 \cos s + A_2 \cos 2s + \&c., \\ + B_1 \sin s + B_2 \cos 2s + \&c., \end{array}$$

where

F = height of water.

s = hour-angle of sun.

The coefficients A_0, A_1, A_2, B_1, B_2, &c., being found by well-known formulæ, they are again expressed, by Fourier's Theorem, as follows:—

$$A_n = a_0 \begin{array}{l} + a_1 \cos u + a_2 \cos 2u + \&c., \\ + b_1 \sin u + b_2 \sin 2u + \&c., \end{array}$$

where u passes through all its changes in a fortnight, and the coefficients are calculated in a similar manner.

The known theoretical formulæ for the Diurnal and Semidiurnal Tides, expressed in terms of parallax, declination, lunar and solar hour-angles, are now converted into functions of the true and mean anomaly and of the sun's hour-angle, and finally into simple functions of s and u. These expansions are now compared, term by term, with the terms of the tidal expansions found by means of Fourier's Theorem, and the final Lunar and Solar Tidal Coefficients calculated out with ease.

Although the short period of observation at Port Kennedy (23 days) renders this method of discussion not much more valuable than the usual method of H. W. and L. W. observations, I have developed it at length in the hope of applying the method to more complete series of Arctic Tides, which I hope shortly to lay before the Royal Society.

In developing this method I found it necessary to make use of the following series, for which I am indebted to my friend Mr. Benjamin Williamson, F.T.C.D.:—

$$\cos ax = \frac{\sin(a\pi)}{\pi}\left[\frac{1}{a} + 2a\,\middle|\,\begin{array}{l} \dfrac{\cos x}{1^2-a^2} \\ -\dfrac{\cos 2x}{2^2-a^2} \\ +\dfrac{\cos 3x}{3^2-a^2} \\ -\dfrac{\cos 4x}{4^2-a^2} \\ +\ \&c. \end{array}\right.$$

$$\sin ax = \frac{2\sin(a\pi)}{\pi}\left|\begin{array}{l} \dfrac{\sin x}{1^2-a^2} \\ -\dfrac{2\sin 2x}{2^2-a^2} \\ +\dfrac{3\sin 3x}{3^2-a^2} \\ -\dfrac{4\sin 4x}{4^2-a^2} \\ +\ \&c. \end{array}\right.$$

II. "Studies in the Chinoline Series.—I. Transformation of Leucoline into Aniline." By Prof. JAMES DEWAR. Communicated by Prof. A. W. WILLIAMSON, Foreign Secretary of the Royal Society. Received February 19, 1877.

In a previous research* on the pyridine series of bases the formation and properties of dicarbopyridinic acid were described. This acid derivative is related to pyridine in the same manner as phthalic acid to benzol. It was then pointed out that the members of the pyridine and chinoline series bear to one another a similar relation to that of benzol and naphthaline, the following analogies being given:—

$\left\{\begin{array}{l} C_2H_2 \\ C_2H_2 \\ C_2H_2 \end{array}\right.$	$\left\{\begin{array}{l} C_6H_4 \\ C_2H_2 \\ C_2H_2 \end{array}\right.$	$\left\{\begin{array}{l} C_6H_4 \\ C_2H_2 \\ C_6H_4 \end{array}\right.$	$\begin{array}{l} C_2H_2 \\ CN\,H \\ C_2H_2 \end{array}$	$\begin{array}{l} C_5H_3N \\ C_2H_2 \\ C_2H_2 \end{array}$
Benzol.	Naphthaline.	Anthracene.	Pyridine.	Chinoline.

An extension of the work was promised in support of these theoretical relations.

Our knowledge of the chinoline series has made little progress since the masterly and exhaustive investigation of Greville Williams†, proving the isomerism of the tar and cinchona bases. The relations of these bodies are still very obscure, owing to the great stability of the bases preventing the formation of derivatives of a simpler type. Indeed some of the most interesting products obtained from these bases, such as the blue colouring-matter cyanine, have a more complicated structure, all attempts to reach gradationally simpler substances of known constitution having been unsatisfactory. Gerhardt remarked that "En général les réactions de la quinoleine sont fort peu nettes"*. It is the object of this communication to render the relations of this substance a little more definite.

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* "On the Oxidation Products of Picoline," Trans. Royal Soc. Edinb. vol. xxvi.

† Trans. Royal Soc. Edinb. vol. xxi.

In the paper above referred to it was shown that pyridine might be obtained by the distillation of dicarbopyridinic acid with soda-lime; and as the acid had been made from the oxidation of picoline this may be regarded as a means of passing from the six- to the five-carbon base. It seemed, therefore, important to ascertain if the members of the chinoline series yielded on oxidation any similar acid, and if a simpler base could be obtained by subjecting it to a like treatment.

In order to carry out this investigation, a quantity of the crude high boiling-point bases occurring in coal-tar was procured, and after repeated fractionation on the large scale was treated in the following manner, to ensure its freedom from pyrrols, phenols, and high boiling-point hydrocarbons. The bases were dissolved in strong nitric acid, separated by means of soda, distilled, and again treated with nitric acid. The addition of small quantities of arsenious acid to the solution was continued for some hours. The bases now liberated from the solution of the nitrate were fractionally distilled, and the specimens boiling between 220° C. and 240° C. were regarded as tolerably pure leucoline. It has been satisfactorily proved by Greville Williams that no amount of fractional distillation will yield chemically pure members of this series; so that this product must be regarded as a mixture of leucoline and iridoline, the names given to the bases in coal-tar isomeric with the chinoline and lepidine obtained from cinchona. In various experiments specimens were used which had been more carefully fractionated, the boiling-point not varying more than 2° C.; but the products obtained were in all cases similar.

The mode in which the oxidation was conducted was similar in all respects to that described in my former paper; but the high boiling-point of the base and the rapidity of the action rendered the use of the condenser unnecessary. The general method of working was as follows:—129 grams of leucoline were dissolved in an equivalent of sulphuric acid and the solution diluted to 600 cubic centimetres. This was then divided into three equal parts, to each of which was added a hot solution of 100 grams of permanganate of potash in about a litre of water, with constant stirring so as to avoid too vigorous an action. In a few minutes the whole of the permanganate is reduced, and the solution, which ought to be neutral, having been made slightly alkaline, is filtered from the oxide of manganese and evaporated to a small bulk, when a large proportion of the sulphate of potash crystallizes on cooling. The mother-liquor was next carefully acidulated with dilute sulphuric acid, when a resinous mixture of acids separates. The crude acids may also be obtained as potash salts by evaporating to dryness the product of the reaction and extraction with alcohol. The aqueous solution of these potash salts, on the addition of acetate of lead, yields a white insoluble lead salt from which the acids may also be separated.

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* 'Traité de Chimie Organique,' par M. C. Gerhardt, tom. iv.

The crude mixture of acids thus obtained, boiled with water for a considerable time and filtered from an oily matter, yields, after treatment with a little animal charcoal, crystals of a well-defined acid.

Leucolinic Acid, $C_9H_9NO_3$.—The crystalline substance obtained as above described is mainly composed of this acid, which may be obtained in colourless plates or needles by a few crystallizations. The acid melts at 162° C., is slowly volatilized when heated to 110° C. in a current of hydrogen, and communicates a characteristic aromatic odour to the vapour from a hot aqueous solution. The acid may be most conveniently crystallized from an aqueous solution, being slightly soluble in cold water. Although soluble in alcohol and ether the acid is generally rendered slightly coloured, from an apparent oxidation, when crystallized from these menstrua.

The salts of this acid are for the most part soluble, the more notable exceptions being the lead, mercurous, and ferric salts. The silver salt crystallizes in fine needle-shaped crystals.

Analyses of different specimens of the acid and silver salt yielded the following results:—

Acid.	I.	II.	III.
Weight of acid taken	0·1625	0·3345	0·2315
Carbonic anhydride produced..	0·3620	0·7450	0·5110
Water	0·0790	0·1595	0·1060

Calculated centesimally these figures give:—

	I.	II.	III.	$C_8H_9NO_3$.
Carbon	60·75	60·74	60·20	60·33
Hydrogen	5·40	5·30	5·09	5·02

Silver Salt.	I.	II.	III.
Weight of salt taken	0·5115	0·3206	0·4925
Carbonic anhydride produced..	0·7080	0·4390	0·6933
Water produced	0·1300	0·0850	0·1276
Silver	——	——	0·1868

Calculated centesimally these figures give:—

	I.	II.	III.	$C_9H_8AgNO_3$.
Carbon	37·73	37·34	38·39	37·76
Hydrogen	2·82	2·94	2·88	2·79
Silver	....	....	37·93	37·76

A quantity of the silver salt, weighing 0·1274 gram, ignited, left 0·04815 gram of silver, corresponding to 37·79 per cent.

Nitrogen Determination.—The nitrogen was found by Gottlieb's method to bear to the carbonic anhydride produced the volume ratio of 1 to 18·4 and 1 to 18·8.

An absolute determination of the nitrogen was made with the following results:—

Weight of acid taken	0·1087 gram.
Volume of nitrogen at 0° C. and 760 millimetres pressure....	7·37 cubic centimetres.
Weight of nitrogen............	·0092 gram.

This gives the percentage of nitrogen as 8·5 instead of 7·8, as required by theory. The acid used in this experiment was not quite pure, and the quantity taken too small for an accurate determination.

Equivalent of the Acid.—A quantity of the acid, weighing 0·445 gram, titrated with pure caustic soda solution, every 1·73 cubic centimetre of which contained 23 milligrams of sodium, required 4·28 cubic centimetres of this solution to neutralize the quantity of acid taken. Hence the equivalent of the acid is 179.

Professor Liveing has kindly determined the following crystallographic constants:—

"The acid crystallizes from solution of ether in tufts of thin plates which show the form of fig. 1.

"The face *a* is the only one which is largely developed. They cleave readily in plane *c* at right angles to *a*. The faces *b*, *b'* are equally inclined to *a*, their normals making an angle of about 53° with that of *a*. The normals to *b*, *c* make an angle of about 69°. The normal to face *d* is inclined to that of *a* at about 42°.

"Many of the crystals do not show the faces *d*, but are terminated by faces parallel to *c*. Some show some smaller facets at the apex, barely discernible.

"There are occasionally twin crystals, with the twin face parallel to the edge *b* and at right angles to *a*.

"The crystals appear to belong to the oblique prismatic system, with the inclined axes in a plane parallel to *a*.

Fig. 1.

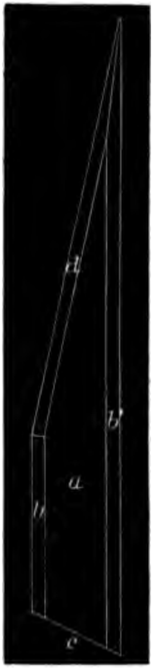
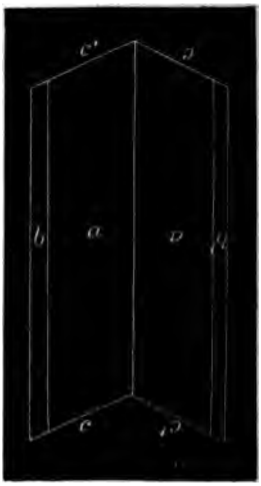


Fig. 2.



"The uncertainty about the measurements is due to the minuteness of all the faces except *a*, and will perhaps be overcome when a fresh crop of crystals is obtained."—G. D. L.

The acid may be regarded as formed by one or other of the following reactions:—

$$(1)\ C_9H_7N+O_2+H_2O=C_9H_9NO_3$$

$$(2)\ C_{10}H_9N+O_6 \quad =C_9H_9NO_3+CO_2;$$

but (1) probably represents the chief source of the acid. Whether perfectly pure leucoline free from traces of iridoline would give the acid must, however, in the mean time remain an open question, as large quantities of material are required to procure the pure base. The yield of acid only amounts to three per cent. of the bases used.

Decompositions of the Acid.

Action of heat.—The acid heated rapidly above its melting-point becomes dark brown, evolves water, and gives an oily sublimate which ultimately solidifies, leaving a mass of porous carbon. The acid sublimate, which probably contains the anhydride, boiled with water regenerates the acid.

Action of soda-lime.—The acid heated with soda-lime to a low red heat leaves a deposit of carbon and yields an alkaline oily distillate. This oil is soluble in dilute hydrochloric acid, and gives the characteristic reactions of pyrrol. The solution evaporated leaves the hydrochlorates of the bases as a crystalline mass, which, after several solutions and evaporations with dilute hydrochloric acid, is freed from pyrrol. The hydrochlorates of the basic substances produced were now heated with potash, when the bases distilled as a colourless oil. This oil has all the physical and chemical characters of pure aniline.

In order to demonstrate the transformation of leucoline into aniline the crude product of the oxidation may be used instead of the isolated acid. A cubic centimetre of leucoline dissolved as sulphate, treated with a solution of 3 grams of permanganate of potash, filtered, evaporated to dryness, and the residue mixed with some soda-lime, yields on distillation sufficient aniline to give all its characteristic reactions. If potash-lime be used instead of soda-lime, the product of the distillation is found to be almost free from pyrrol.

In order to ascertain if any base other than aniline was present, 3 grams of the acid was heated with potash-lime and the distillate collected in dilute hydrochloric acid. This was fractionally precipitated by chloride of platinum, when 0·1248 gram of the first fraction gave 0·0425 gram of platinum, equal to 34·05 per cent., aniline requiring 32·99 per cent. The second fraction, 0·2101 gram, gave 0·0688 gram of platinum, equal to 32·74 per cent.

The only substance associated with aniline seems to be ammonia, as appears from the following experiment, undertaken to ascertain whether any nitrile bases occurred in the distillate. The distillate from other 3 grams of acid was collected in nitric acid, and fragments of nitrite of potash added, until all the aniline was decomposed. The solution after being boiled was treated with caustic soda and distilled. The distillate was slightly alkaline, and gave a platinum salt, of which 0·216 gram yielded 0·0954 of platinum, equal to 44·16 per cent. of platinum, the double chloride of platinum and ammonium requiring 44·23 per cent. The mother-liquor yielded nothing apparently but the ammonium salt. The acid on heating with potash-lime mainly undergoes the following reaction:—

$$C_9H_9NO_3 = C_6H_7N + CO_2 + H_2O + C_2.$$

When the crude mixture of potash salts resulting from the oxidation is taken, instead of the pure acid, and distilled with soda-lime, there appears to be associated with the aniline and pyrrols a small quantity of a nitrile base having a lower boiling-point than leucoline. If the pure potash salt is heated alone it fuses, aniline distils along with the vapour of water, and a mixture of carbonate of potash and carbon is left. Thus

$$2C_9H_8KNO_3 = 2C_6H_7N + H_2O + K_2CO_3 + CO_2 + C_4.$$

If the potash salt is fused with excess of potash, ammonia is evolved and salicylic acid is found in solution. Probably anthranylic acid is the first product, and this is subsequently changed into ammonia and salicylic acid, viz.—

$$(1)\ \underset{\text{Leucolinic.}}{C_9H_9NO_3 + 3H_2O} = \underset{\text{Anthranylic.}}{C_7H_7NO_2 + 2CO_2 + H_8}.$$

$$(2)\ \underset{\text{Anthranylic.}}{C_7H_7NO_2 + H_2O} = \underset{\text{Salicylic.}}{C_7H_6O_3 + NH_3}.$$

A similar series of reactions take place when indigo is carefully treated with fused potash. The formation of salicylic acid explains the origin of the ammonia found associated with the aniline when the acid is heated with potash-lime.

A solution of the acid in glycerine, heated to the boiling-point of the latter, gives a distillate of glycerine, aniline, and a substance in small quantity having the characters of indol. No free carbon is separated in this reaction. That indol or an isomeric body is likely to occur amongst the products of decomposition appears from the following likely decomposition:—

$$\underset{\text{Leucolinic Acid.}}{C_9H_9NO_3} = \underset{\text{Indol ?}}{C_8H_7N} + H_2O + CO_2.$$

This reaction is rendered probable from the relations of indol and aniline, indol being readily obtained from the reduction of isatine, and isatine yielding aniline, carbonate of potash, and hydrogen on fusion with potash. The relations of chinoline, leucoline, and indol will be fully investigated in the second part of this paper, after I have had an opportunity of examining the isomeric cinchona bases. In the mean time it may be remarked that pyrrol and pyridine have a similar relation to indol and chinoline, thus—

$$\underset{\text{Pyrrol.}}{\left\{\begin{matrix} C_2H_2 \\ NH \\ C_2H_2 \end{matrix}\right.} \qquad \underset{\text{Pyridine.}}{\left\{\begin{matrix} C_2H_2 \\ CN\,H \\ C_2H_2 \end{matrix}\right.} \qquad \underset{\text{Indol.}}{\left\{\begin{matrix} C_6H_4 \\ NH \\ C_2H_2 \end{matrix}\right.} \qquad \underset{\text{Chinoline.}}{\left\{\begin{matrix} C_6H_4 \\ CN\,H \\ C_2H_2 \end{matrix}\right.}$$

In my paper before referred to it was suggested that, "considering the stability and mode of formation of these bases, it is not at all improbable that they may be produced by the simultaneous action of acetylene and its derivatives on hydrocyanic acid; thus as three molecules of acetylene condense and form benzol, so may two molecules of acetylene and one of hydrocyanic acid condense and produce pyridine."

A synthetical experiment of the kind suggested has been executed by Mr. Ramsay*, who finds that by transmitting a mixture of acetylene and hydrocyanic acid through a red-hot tube bases were unquestionably produced. Pyrrol, which may be so readily identified by means of its characteristic reaction with fir wood moistened with hydrochloric acid, may be formed synthetically by substituting ammonia for the hydrocyanic acid in the above experiment. The acetylene employed, however, contained a small quantity of bromide of vinyl, and it is possible the reaction may have taken place between that substance and ammonia. Only a small quantity of pyrrol is formed in this reaction, the principal substance formed being cyanide of ammonium; and the success of the experiment seems to depend on the maintenance of a carefully regulated temperature and a certain extent of porous surface. These and similar reactions are under investigation.

The theoretical bearings of this investigation have not been touched upon in the present paper, as an extensive research will be necessary before structural relations can be predicted with any certainty.

I am greatly indebted to Mr. W. F. Sell, B.A., and Mr. A. Scott, B.Sc., assistants in the Chemical Department, for aid in the course of the investigation.

Laboratory, Cambridge University.

III. "On the Density of Solid Mercury." By Prof. J. W. Mallet, University of Virginia. Communicated by Prof. Stokes, Sec.R.S. Received February 22, 1877.

I have lately taken advantage of a heavy fall of very cold and finely pulverulent snow, well adapted to the preparation of freezing-mixtures, to redetermine, with accuracy I believe, the density of mercury in the solid state and at a definite temperature.

Such redetermination was not superfluous, as appeared from a collation of the statements to be found in various standard works. In the tables of specific gravities compiled by Prof. F. W. Clarke, and published by the Smithsonian Institution†, there are four authorities quoted, with the numbers given by these, as follows:—

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* "On Picoline and its Derivatives," Phil. Mag. 1876, vol. ii. p. 269.

† "The Constants of Nature.—Part I." Smithsonian Miscell. Coll. 255, Washington 1873, p. 24.

	Sp. gr. of solid mercury.
Schulze	14·391
Biddle	14·485* at —60° C.
Kupffer and Cavallo	14 (approx.)
Joule	15·19

The last of these numbers, on reference to the original paper†, turns out to represent no actual experiment with mercury itself, but is the density *calculated* for this metal from the examination of a number of amalgams. Kupffer and Cavallo do not profess to give the exact density, but merely state it as about 14, the number apparently resting on no special experiment, though I have not been able to verify this by reference to their paper‡. The only other apparently independent statement I have met with occurs in the 'Annuaire du Bureau des Longitudes' for 1876 (p. 385), where the density 14·39 is given on the authority of Rivot; but I have not been able to find any reference to a paper by him bearing on this or any analogous point, and it seems probable that we have here only a reproduction of Schulze's result. In different handbooks of chemistry and physics numbers between 14 and 15 are given as approximations, but with no other authority than some of the above. Some of the best and most recent works simply state that mercury undergoes considerable contraction in freezing. Hence our knowledge on this subject appears hitherto to have rested on the experiments of Schulze and Biddle, both of which date back to the early years of the present century. Schulze's paper was published in 'Gehlen's Journal,' vol. iv. p. 434, and therefore about 1807 or 1808, and Biddle's§ belongs to the year 1805. I have had access to neither; but the character of the instrumental means (balances, thermometers, &c.) generally available at the time the experiments were made, and the then imperfect knowledge of the constants needed for corrections to be applied, make it unlikely that very exact results could have been obtained. Biddle alone seems to have noted the temperature of the frozen mercury, and Brande‖ expresses doubt that this was determined with much accuracy. The temperature —60° C., if correctly quoted, is in itself somewhat improbable.

The method adopted in the experiments lately made in this laboratory was the following:—

(1) A specific-gravity flask was prepared from a large cylindrical pipette by closing in and smoothly rounding in the flame of the lamp one end of the cylinder, while the tube remaining attached to the other end was cut short and united by fusion to a second pipette of like shape but

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* 14·465 as quoted by Brande in his 'Manual of Chemistry.'

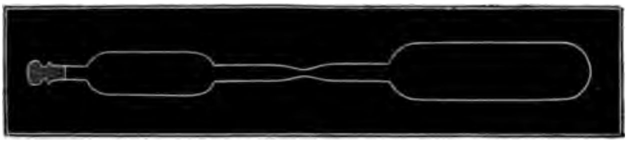
† Chem. Soc. Journ. [2] i. p. 387.

‡ Quoted at second hand from Playfair and Joule, "On Atomic Volume and Specific Gravity," Chem. Soc. Mem. 2 (1845), p. 401, and 3 (1848), p. 57.

§ Nicholson's Journal, vol. x. p. 253, and Tilloch's Philos. Mag. vol. xxx. p. 134.

‖ Manual of Chemistry, vol. i. p. 970.

smaller size, the upper and open end of the shortened tube of which was fitted with a small carefully ground glass stopper. The neck between the larger and smaller cylinders was drawn down to a small bore (about 2 millims.), and at this narrowed part a fine line marked round it with a diamond. The shape of the vessel is shown in the annexed sketch, on a linear scale of one half the real size. The principal cylinder held about 58 cubic centimetres, and the small reservoir above 25 cub. centims. The whole vessel weighed about 46 grammes.



It enabled the experiments to be carried out with more than half a kilogramme of frozen mercury.

(2) This vessel having been accurately weighed when empty and dry, its capacity up to the mark was ascertained by filling it to this point with pure water at exactly 4° C., keeping it immersed for some time in a large mass of water at this temperature before making the final adjustment to the mark, wiping the outside dry, allowing the whole to acquire the temperature of the balance-case, and carefully weighing. The result of this direct calibration, deducting the weight of the vessel, was 59·7323 grammes or cubic centimetres at 4°.

(3) It was checked by emptying and drying the vessel, filling it to the mark with pure mercury at 0° C., the temperature being secured by keeping the whole surrounded by melting ice long enough to obtain perfect steadiness of position of the mercury, and weighing after the temperature of the balance-case had been regained. The mercury weighed 811·9997 grammes.

(4) The vessel was now surrounded by steam, and the mercury again brought to the mark, the temperature actually attained being 99°·5 C. (corrected for pressure). Allowed to cool down to the temperature of the balance-case, and again weighed, the mercury was found =799·7032 grammes. From the last two weighings the coefficient of cubical expansion for 1° C. of the glass used was, by the usual formula (taking absolute expansion of mercury from 0° to 100 =·018153, as determined by Regnault), found =·000027346.

(5) If now the density of mercury at 0° as referred to water at 4° be taken at 13·596 (Regnault), the weighing obtained in (3) gives the capacity of the vessel up to the mark at 0° =59·7234 cub. centims., or, applying the above coefficient of expansion of glass, 59·7300 cub. centims. at 4°. The mean of this value and that obtained in (2), $=\frac{59{\cdot}7300+59{\cdot}7323}{2}$ =59·7311 cub. centims., was taken to represent the true capacity of the vessel at 4°.

(6) The freezing-mixtures used were prepared by cooling commercial hydrochloric acid (sp. gr. =1140) in the snow out of doors, the temperature of which, as well as of the air, was on the first day about −9° C., but on subsequent days rose to about −5°, mixing equal weights of this cooled acid and of snow, using separate portions of this first mixture to cool more acid and snow, and finally bringing together these last.

It soon appeared that little advantage was gained by trying to cool the snow, on account of its very low conducting-power in such a loose porous condition; and in the later experiments the temperature of the acid alone was lowered before the final mixture with snow. The glass vessels containing the mixtures were large enough to maintain the cold required for a long time, and steadiness of temperature was secured by surrounding them on all sides with a layer of cotton wadding, kept in place by stiff brown paper, and by conducting all the operations out of doors in the unusually cold atmosphere prevailing at the time.

(7) In determining the temperature of the freezing-mixtures an alcohol thermometer was used, graduated to single degrees, and admitting of half a degree being read; but the scale being found by no means accurate, its absolute readings were altogether discarded. By comparison with a good mercurial thermometer at three or four points between −10° and +40° C., and calculation from Is. Pierre's coefficients, the real length of a degree on the part of the stem corresponding to −40° was determined; and the temperature of fusion of the mercury being accurately noted and assumed = −38°·85 C., as determined by Balfour Stewart*, the addition or subtraction of four or five degrees, as above obtained, gave all the other temperatures observed.

(8) The above weighings and all others to be mentioned were made with an excellent balance by Becker, carefully adjusted and tested at the outset. With a load of a kilogramme in each pan a difference of weight of $\frac{1}{10}$ milligramme can be detected, and $\frac{1}{5}$ milligramme may be fully relied upon. All weighings were reduced by calculation to the corresponding results *in vacuo*, the temperature and pressure of the atmosphere being noted on each occasion; and the results quoted are those thus corrected.

(9) The specific-gravity flask was now filled with alcohol (at one time absolute, but which, by long keeping in the laboratory and occasional opening of the bottle, had absorbed some moisture, and was really about 95 or 96 per cent.), and three weighings were obtained after the liquid had been carefully adjusted to the mark at temperatures close to the freezing-point of mercury†.

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* With an air-thermometer (Proc. Roy. Soc. 1863, vol. xii. p. 674).

† The alcohol, as afterwards mercury, was brought to near the required temperature before introduction into the final freezing-mixture, and a separate small portion in a tube was similarly cooled, to be used in filling up to the mark if necessary. The stopper was carefully inserted as soon as the adjustment of the liquid was secured, so as to avoid any loss by evaporation.

Applying the correction for capacity of vessel at the respective temperatures, the three results were :—

cub. centims.			grms.
59·6625	of alcohol at	−37° C.	=50·7010
59·6600	„ „	−40°	=50·8316*
59·6576	„ „	−42°·5	=50·9092

or, reducing to one common weight,

cub. centims.			grms.
117·6752	of alcohol at	−37°	=100
117·3679	„ „	−40°	=100
117·1843	„ „	−42°·5	=100

(10) Taking the difference between (*a*) the first and second, (*b*) the second and third, and (*c*) the first and third of these numbers, and dividing each difference by the number of degrees in the interval of temperature, we get as the change of volume of 100 grammes of alcohol for 1° C. :—

	cub. centim.
From (*a*)	·1024
(*b*)	·0734
(*c*)	·0893

and the mean of these (·0884) may be taken to represent the coefficient for 1° C. within a range of a few degrees either side of the freezing-point of mercury. Using this coefficient to reduce the three weighings to their corresponding values for the same temperature, say —39° C., we have

grms.	cub. centims.
100 of the alcohol in question at −39°	=117·4984
„ „ „ „ „	=117·4563
„ „ „ „ „	=117·4937

the mean of which is 117·4828 cub. centims.

(11) The specific-gravity flask having been emptied and dried, 558·9353 grammes of mercury was introduced, the metal having just previously been purified by careful treatment with dilute nitric acid, washing with water, and quiet distillation from a glass retort. Filling up with the same alcohol as that used in the above experiments, and which had been kept in a well-stoppered bottle, the flask was gradually cooled, and finally, in the last freezing-mixture, the mercury frozen, and the alcohol brought exactly to the mark, taking care that it became and remained quite stationary, while during the freezing of the mercury the change of volume was very rapid and easily observable. The temperature having been noted when the final adjustment was made, the little flask was set aside, stoppered, until it could be washed off and dried, and was then allowed

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* This weighing was not quite so satisfactory as the remainder; the temperature a little doubtful.

to acquire the temperature of the balance-case, and weighed. Three such experiments gave, aside from the weight of the flask itself,

			grms.
A.	Mercury + alcohol at	−39° C.	=576·2029
B.	„ „ „	−41°·5	=576·2522
C.	„ „ „	−42°	=576·2639

Deducting the mercury, the quantity of which remained constant throughout, it appears that the flask contained of alcohol:—

		grms.
A.	at −39°	17·2676
B.	„ −41°·5	17·3169
C.	„ −42°	17·3286

From the data in (10) these weights represented at the respective temperatures the following volumes:—

	cub. centims.
A	20·2865
B	20·3061
C	20·3122

From the data in (4) and (5) we get the capacity of the flask up to the mark at the same temperatures:—

	cub. centims.
At −39°	59·6609
„ −41°·5	59·6568
„ −42°	59·6560

Subtracting the volume of alcohol in each case, that of mercury was,

	cub. centims.
In A	39·3744
„ B	39·3507
„ C	39·3438

Hence the specific gravity as obtained

In A	14·1954	at − 39°
„ B	14·2034	„ − 41°·5
„ C	14·2064	„ − 42°

(12) By comparing these numbers in pairs, we have as the difference apparently due to a difference of temperature of 1° C.[*]:—

From A and B	·0032
„ B and C	·0060
„ A and C	·0037

of which the mean is ·0043.

Reducing, by using this coefficient, the above results to a single temperature, and adopting that of the fusing-point of the metal as determined by Balfour Stewart, we get

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[*] Of course really including errors in determination of weights and temperatures.

From A ...... 14·1948 at −38°·85 C.
„ B ...... 14·1920 „ „
„ C ...... 14·1929 „ „

and, as a final mean of these three, 14·1932 *as the number representing the density of solid mercury at its fusing-point as referred to water at 4°C. taken as unity.* I think this result (which, it will be seen, differs considerably from the figures hitherto quoted) may be fairly accepted with confidence.

In these experiments most of the weighings were made by Adjunct Professor Dunnington, and the freezing-mixtures were managed, at no small cost of personal discomfort, by Messrs. Bryan and Memminger, students in this Laboratory. To these gentlemen my thanks are due.

IV. "The Automatic Action of the Sphincter Ani." By W. R. Gowers, M.D., Assistant Physician to University College Hospital. Communicated by J. S. Burdon Sanderson, M.D., F.R.S., Jodrell Professor of Human Physiology in University College, London. Received February 24, 1877.

The observations described in the following paper had for their object the determination of the form of the reflex or automatic action of the sphincter ani of man when voluntary power over it is lost. This reflex action is believed, from the researches of Masius*, to depend on an "ano-spinal centre," situated in the lumbar enlargement of the spinal cord, controlled in health by higher (encephalic) centres. It appears, however, to be very uniform in its character in various conditions, the most conspicuous common character of which is the entire loss of voluntary power.

The larger number of observations were made on a man who, by a violent fall on the sacrum, had apparently injured the posterior roots of all the sacral nerves and both roots of the lowest sacral nerves. A depression existed over the lower part of the sacrum. Sensibility to touch and pain was lost in all parts supplied by branches from the sacral plexus, the limitation being exact. There was no muscular paralysis or loss of nutrition except in the levator ani, the sphincter ani, and the sphincter vesicæ, all of which were paralyzed to the will. The anus and the mucous membrane of the rectum were quite insensitive. There was no evidence of any injury to the spinal cord; with this, indeed, the symptoms were incompatible. It would thus appear that the only lesion was a division of the direct communication between the sphincter and the cord. Other observations were made on two patients with paraplegia, due probably to disease of the dorsal region of the spinal cord, in whom there was reason to believe that the lumbar enlargement of the cord was free from material damage. In each case there was not the slightest voluntary power to retain the contents of the rectum. It was found that in each the condition of the sphincter was essentially the same, and that it was in a state of high reflex activity. The most uniform results were obtained in the case of injury to the sacral nerves. Finally these results were compared with those obtained by the same method when voluntary power was intact.

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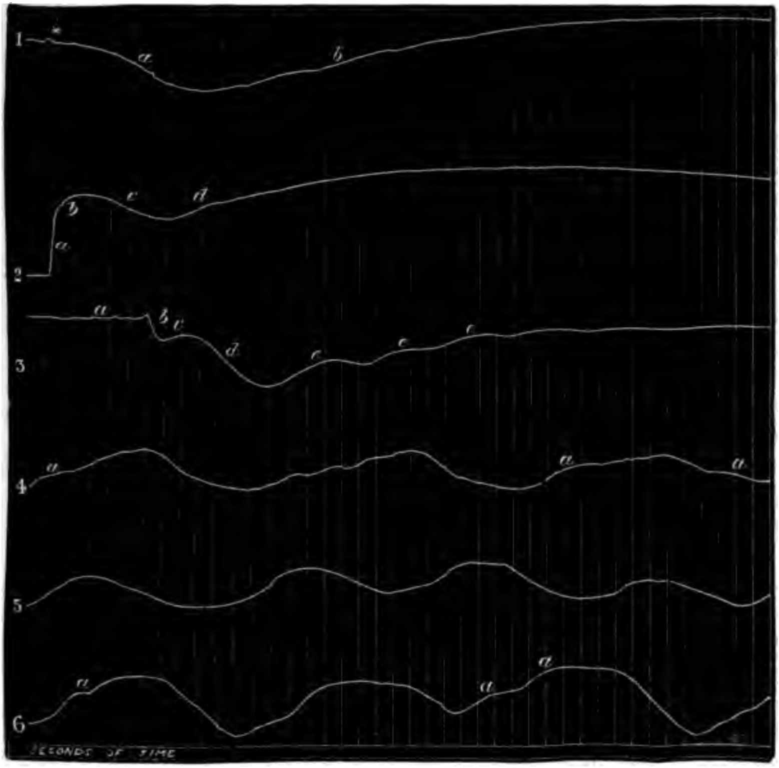
* Bull. de l'Académie Royale de Belgique, 1867, t. xxiv. p. 312, and Journal de l'Anatomie et Physiologie, 1868, p. 197.

The instrument employed was a small cylinder of thin india-rubber, supported at each end on a wooden plug, the anterior extremity of which was conical, to facilitate its introduction. The chamber within communicated by means of a flexible tube with the cavity of a tympanum provided with a writing lever, by which the variations in the pressure were recorded on a revolving drum (Marey's cardiograph). A small metal tube passed through the middle of the cylinder, by which air could be injected into the rectum without disturbing the instrument. A smaller instrument, the rubber cylinder of which was only one inch in length, was used to ascertain the effect produced by different portions of the lower end of the bowel.

The first fact ascertained was that in each case, although the incontinence of fæces was complete, the sphincter was habitually in a state of continuous, slightly varying contraction. That this tonic state was not due to the presence of the instrument within the anus is shown from the fact that it existed before the introduction of the instrument, and that any irritation of the anus, as by movement of the instrument, produced a well-defined effect of a different character. The same tonic contraction is shown in every tracing obtained. After a disturbance caused by the introduction of the instrument was over, the pressure continued nearly the same throughout, being marked only by a few very slight and irregular variations.

This continuous contraction was, however, inhibited by any irritation applied to the mucous membrane of the rectum. Such an irritation was readily effected by the injection of a small quantity of air into the rectum. The result of such an injection is shown in the tracing (fig. 1). A rapid fall in pressure occurred (*a*), due to the relaxation of the sphincter, which was in some cases so complete that the instrument fell out. After a brief period of complete relaxation, contraction occurred, at first slight, and then slowly increasing, indicated by the rise of the lever (*b*), until the original pressure was attained. In most cases the rise was to a higher point than the original pressure, and a subsequent slight fall occurred until the initial pressure was reached.

When the irritation was produced by a solid body, a slight brief increase in the contraction preceded the relaxation of the sphincter. This was seen, for instance, when the wooden head of the instrument was pressed down upon the sphincter. The most ready way in which the effect was produced was by making the patient cough. This initial contraction was also very marked on the irritation produced by the introduction of the instrument, the invariable effect being the succession of changes of pressure which are shown in the tracing (fig. 2). The vertical rise (*a*) is the effect of the pressure of the sphincter to which the instrument is suddenly exposed on its introduction; the top of this line represents the amount of pressure exerted by the sphincter before the introduction of the instrument. This is followed by a slight rise (*b*), succeeded by a considerable and rather quick fall (*c*), which fall is again succeeded by a rise (*d*) to a point a little higher than that from which the lever fell, as in the effect of the injection of air.



In the slow rise after this inhibition there was often an indication of a tendency to rhythmical action. In some tracings, especially those which show the effect of a cough, this is very distinct (fig. 3). The slight fall (*b*) immediately after the cough is the result of the movement of the instrument lessening the extent to which it is compressed by the sphincter. The lower end of this line represents, therefore, the degree of the previous contraction of the sphincter, and corresponds to the top of the line (*a*) in fig. 1. The slight initial rise (*c*) precedes a considerable fall (*d*), and is succeeded by a slow rise, in which there are secondary waves of rhythmical variation (*e e e*). No such variation could be traced in the continuous contraction which followed.

Goltz* observed that in dogs, after division of the dorsal cord, a rhythmical action was caused by the presence within the sphincter of any foreign body. In man it does not appear that the presence of an unirritating foreign body within the anus, provided it is kept still, constitutes stimulation or excites any reflex action. No rhythmical variations were, as a rule, observed in the tonic contraction. In one case of disease of the dorsal spinal cord, however, the mere presence of the instrument caused sometimes a lengthened inhibition, at the end of which some rhythmical contraction occurred. But it was found that, in every case, the continuous injection of a jet of air into the rectum developed, very uniformly, a rhythmical action. The rapid fall which occurred immediately after the commencement of the injection was succeeded by a rather quick rise, followed by another fall, and then a corresponding rise, and so on in successive alternations. Thus a continuous series of nearly uniform curves was obtained (figs. 4, 5, 6). The height reached by the lever in these variations was in some instances the same as that of its continuous contraction, but in other cases it was less high. The cause of the rhythmical variation appeared to be, in part at least, the alternating accumulation of the air within and its escape from the rectum during the contraction and relaxation of the sphincter, the accumulation causing the inhibition of the sphincter, which permitted the escape of air. A difference noticeable in the form of these curves will be considered in speaking of their duration.

In some instances the rhythmical action was long in being developed, the first effect of the continuous injection being a complete inhibition of considerable length, succeeded by the intermitting contractions.

Goltz observed that a powerful sensory impression on the hind legs of a dog inhibited the rhythmical contractions of the sphincter. In the case of injury to the sacral nerves no such inhibition could be obtained by strong faradic stimulation of the skin of the lower part of the abdomen, on which sensation was intact.

The effects of a voluntary effort, a sigh, and a cough were observed to ascertain if there was any consentaneous contraction of the sphincter; but none could be observed.

Careful measurements were made of the duration of the several events in the reflex action above described.

On every form of stimulation it was found that a period elapsed after the commencement of the stimulation before there was any change in the degree of contraction. After an injection of air, as nearly instantaneous as could be, the latent period amounted on one occasion to 1·3 second. After a cough it varied from ·8 to 1·2 second, the average of five measurements being just 1 second. The latent period after the introduction of the instrument, before the initial rise, varied from 1 to 1·5 second.

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* Pflüger's Archiv, vol. viii. 1874, p. 479.

The initial rise, when it occurred, was very uniform in its duration. After the cough it varied from 1·1 to 1·5 second; four out of six measurements were exactly 1·5 second. After the introduction of the instrument, the initial rise, in four measurements, lasted 1·3, 1·5, 1·5, and 2 seconds respectively, the mean of the whole being very nearly 1·5 second. In the cases of disease of the cord the initial rise after the introduction of the instrument was rather longer, lasting 2 seconds.

The duration of the subsequent fall varied considerably. After the introduction of the instrument it varied from 3 to 4·5 seconds, three out of five observations being exactly 4 seconds. After a cough the fall occupied from 3·5 to 5·5 seconds. The mean of all the measurements of the fall caused by the mechanical stimulation of the instrument (on cough and introduction) was 4·2 seconds. It was found that the more considerable the fall the longer was its duration. Thus a slight fall caused by traction on the instrument (not included in the above average) lasted just 3 seconds. The initial rise did not exhibit this relation; in the last case it lasted 1·5 second, while the period of latent stimulation was exactly 1 second. In the case of disease of the cord the duration of the fall was 5 or 6 seconds.

The subsequent rise always occupied a much longer period than the preceding fall, varying in duration from 10 to 17 seconds. Only in one instance was it less than 10 seconds, and in that the rise was imperfect and was succeeded by a second fall. The mean of thirteen measurements of the rise after all forms of stimulation, in the case of injury to the nerves, was 13·5 seconds. In the case of disease of the cord it was somewhat longer, varying from 17 to 30 seconds.

The rhythmical variations which in some cases occurred during the rise (fig. 3) were from 4 to 4·5 seconds in duration, and, in the case of injury to the sacral nerves, were very uniform. As there was no corresponding variation in the stimulation, they must be regarded as the expression of a spontaneous rhythm in the action of the sphincter.

The length of the rhythmical contractions which resulted from a continuous injection of air varied considerably, and, as already mentioned, the form of the curves obtained also varied. Some (in each case) were of considerable length, lasting from 12 to 17 seconds. In these the fall was much steeper than the rise (figs. 4 and 6). These curves resembled in this the curve which was obtained on any sudden stimulation, and appeared to be merely a series of such curves, resulting from the intermitting inhibition consequent on the alternate accumulation and escape of air. But on some occasions curves were obtained of a different form and shorter duration (fig. 5). The descent corresponded in inclination with the ascent. The duration of each period in the rhythm was nearly 9 seconds. It is to be noted that this is just double the length of the spontaneous rhythmical variations in the case of the rise after the mechanical stimulation by a cough &c. These more regular curves would appear, then, from their curve and duration, to be the more direct effect of the tendency on the part of the sphincter to rhythmical action under the influence of the continuous stimulation*.

The longest complete inhibition under a continuous injection of air lasted 30 seconds. It is evident that this might easily have been mistaken for permanent relaxation.

A comparison of these results with the action of the sphincter ani under normal conditions corroborated the conclusion which Masius and Goltz drew from their observation upon dogs, that the reflex action and tendency to rhythmical variations is modified and controlled by the higher encephalic centre. No variation in the uniform contraction of the sphincter resulted from either the introduction of the instrument or from its movement by a cough. Inhibition of contraction could, however, be readily produced by an injection of air into the rectum. No initial rise was observed under any circumstances: the inhibition continued during the whole period of a short injection of air, and on the cessation of the injection the pressure quickly rose to its original height. The duration of the several parts of the action differed little from that observed in the other cases. A latent period of 1·5 second was succeeded by a fall of 5 seconds' duration, a period of complete inhibition of 6 seconds, and a subsequent rise which occupied 11 seconds.

As far as could be ascertained, the internal sphincter was alone concerned in this reflex action. The external sphincter appeared to be in each case relaxed.

In the case of injury to the sacral nerves the direct communication between the sphincter and the cord must have been interrupted; the reflex action, if from the cord, can only have taken place through the sympathetic nerves. It is a point for future investigation, suggested by certain points of resemblance between the reflex action of the sphincter and that of the uterus, the intestine, and the heart, whether its action is entirely dependent on the ano-spinal centre in the cord, and also by what mechanism the encephalic centre exerts its influence.

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* The immediate cause of the intermitting inhibition, no doubt, was in each case the intermitting rectal stimulation; but the greater uniformity of ascent and descent, the resemblance in form of these causes to those which were undoubtedly spontaneous, suggests that the tendency to rhythmical relaxation determined the time and form of its occurrence in this case (fig. 5), whereas the longer curves, with steeper fall, were determined more directly by the reflex inhibitory impressions. It is to be noted that the latter often exhibited small secondary waves (as in fig. 6, *a a*), resembling the spontaneous waves (fig. 3, *e e*) in their uniform curve; while the regular waves (as in fig. 4) never exhibited these smaller waves of ultimate rhythm, as they might be called.

I would, however, draw especial attention to the points of resemblance between this reflex action of the internal sphincter and that of the middle coat of the intestine in peristaltic action, which suggest the probability that the action of the sphincter, apart from the will, is under the control of a similiar mechanism, and is indeed only, so to speak, a concentrated and more specialized instance of the action of the transverse fibres of the intestine. The action of the intestine, as well as that of the sphincter, is under central control, being inhibited by the vagus, intensified by the splanchnics. The deliberate character of the reflex action of the sphincter resembles closely the deliberate character of the intestinal reflex.

Increased intestinal contraction, like that of the sphincter, is excited most readily by irritation of the mucous membrane. Moreover Goltz believed that in dogs the muscular coat of the rectum participated in the rhythmical contraction which he observed in the sphincter; and my own observations have shown that the reflex action I have described is not confined to the thickened extremity of the bowel, but can be obtained in a modified form as high as two inches from the lower extremity.

The power of reflex action which is possessed by the whole internal sphincter must be possessed by each bundle of muscular fibres of which it is composed. As the sphincter may be regarded as an aggregation of bundles of fibres, such as are contained in the transverse muscular coat of the intestine, so the latter may be regarded as a serial arrangement of the bundles of which the internal sphincter is composed. An action of each bundle of fibres so arranged, such as we have seen to occur in the sphincter as a whole, must result in peristalsis, in the movement of a contained and stimulating body along the intestine. If each bundle of fibres passes through the same series of successive contractions and relaxations as the sphincter ani, then the curve traced by the action of the latter will represent not merely the condition of one bundle of fibres in successive intervals of time, but also the condition of successive bundles at the same time, and two such curves in apposition will represent a diagrammatic longitudinal section of the intestinal wall. The effect, therefore, of the presence in the intestine of a mass of fæces or other contents would be to cause, first, in the moderately contracted intestinal wall in front of it, an increased contraction, the effect of which would be to prevent the diffusion of the contents along the intestine (which would materially interfere with their movement); secondly, complete relaxation of the next portion of the intestinal wall into which the contents of the intestine could pass; and thirdly, a strong contraction behind, sustained, and moving on the stimulating body, as the initial contraction gave place to relaxation. The process would no doubt be materially modified by the contraction of the longitudinal fibres of the bowel, which would prevent the undue distension of the relaxed portion, and thus assist the transmission onwards both of the contents of the bowel and of the resulting stimulation. The contraction of these longitudinal fibres would also mask the details of the process to external observation. The intermitting contractions of the sphincter under a continuous stimulation may represent the successive waves of peristaltic action when the intestinal contents are abundant. It is further to be noted that the presence of the instrument in the anus, after the effect of its introduction had passed off, was the source of no stimulation, just as contents may be at rest within the bowel, and if they are not moved, and do not irritate the mucous membrane, may excite no peristaltic action.

EXPLANATION OF TRACINGS.

Fig. 1. Effect on contraction of sphincter of the injection into the rectum of a small quantity of air at *. *a*, fall in pressure due to the inhibition of the contraction; *b*, rise due to the slowly returning contraction.

Fig. 2. Effect of the introduction of the instrument. *a*, sudden rise of lever at moment of introduction, due to the exposure of the instrument to the pressure of the sphincter (the top of this line represents the degree of previous contraction); *b*, initial rise due to increased contraction; *c*, fall from partial inhibition; *d*, subsequent contraction, rising to a greater degree than the initial contraction, and subsequently falling slightly.

Fig. 3. Effect of cough. *a*, pressure of tonic contraction of sphincter (the slight irregularities are due to pulse-waves); *b*, fall in pressure, due to the movement of the instrument by the cough; *c*, initial contraction; *d*, relaxation of inhibited sphincter; *e*, *e*, *e*, rhythmical variations in subsequent rise.

Figs. 4, 5, 6. Rhythmical variation in contraction of sphincter under the influence of a continuous injection of air into the rectum. *a*, *a*, *a*, waves of secondary rhythm.

Figs. 1, 2, 3, 4, & 6 are from the case of injury to the sacral nerves. Fig. 5 is from a case of disease of the dorsal region of the spinal cord.

The vertical lines represent seconds of time.

V. "Description of the Process of Verifying Thermometers at the Kew Observatory." By FRANCIS GALTON, F.R.S. Received March 1, 1877.

It may be of interest to describe the method recently adopted at the Kew Observatory of verifying thermometers by comparison at different temperatures with a standard instrument, since a large proportion of the various thermometrical determinations made by English physicists are dependent for their accuracy upon that of the verifications at Kew. Many thousands of thermometers have already been verified by the apparatus about to be described.

Up to the year 1875 the apparatus for this purpose at the Kew Observatory was of the rudest character.

It was simply a glass jar 9½ inches wide and 18 inches deep, filled with hot water and standing on a turntable, in which a brass frame was placed.

The thermometers were attached to this framework, and the observer having well agitated the water with a plunger, read the instruments in succession through the glass as he turned the jar round before him, reading each thermometer as it passed. He first turned it round from right to left, and then back again from left to right. Each thermometer was thus read twice, and the mean of the pair of readings was taken. It is obvious that if the rate of cooling of the water be uniform, and if the thermometers are observed at precisely equal intervals, the mean of every pair of observations would be strictly referable to the temperature of the water at the same moment of time, namely, to that which is half-way between the beginning and end of the entire set. It is needless to point out that these conditions can never be strictly fulfilled, although, notwithstanding the imperfection of the process and the coarseness of the apparatus, the observers acquired much certainty and skill in its manipulation. Still the time occupied was unnecessarily great, and the chance of error, owing to variations in the rate of cooling of the water, was larger than it need be. Partly owing to this latter reason, and partly to the fact that the number of thermometers sent to be tested has considerably increased (being now not less than 3000 annually), I thought it advisable to design and propose to my colleagues of the Kew Committee the construction of an instrument of a much more substantial and adequate character; and to this the Committee assented. I was subsequently indebted for many suggestions to Mr. De La Rue, and also to Mr. R. Munro, of 24 Clerkenwell Green, London, by whom it was finally made. It has now been at work for two years, and its performance is quite satisfactory; experience has in the mean time suggested a few emendations and simplifications, and I will therefore describe the instrument as at present in use.

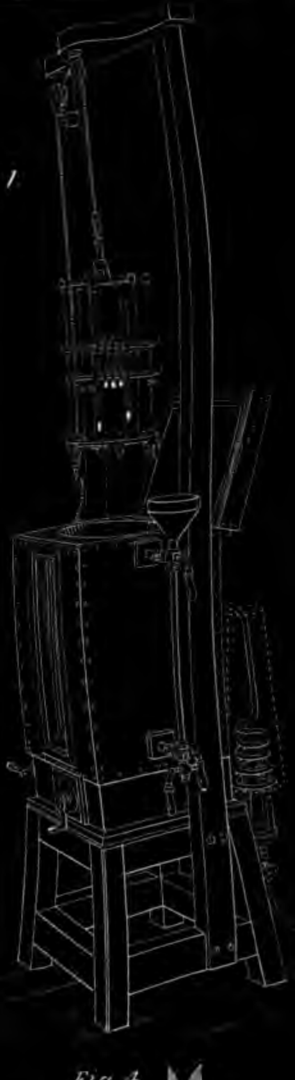
The apparatus (see figs. 1 & 2) consists essentially of four parts:—

(1) A water-vessel.
(2) An agitator, worked by a handle on the outside.
(3) An external heating arrangement.
(4) A frame on which to hang the thermometers, turned by a handle on the outside.

(1) *The Water-vessel.*

This is a cylinder of stout copper, 2 ft. 2 in. high and 1 ft. in diameter. In its base there is a central aperture through which the concentric vertical axes are passed, which respectively carry the agitator and the thermometer frame; the top of the cylinder is entirely open; a vertical slit, 1 ft. 10 in. long and 4½ in. wide, is cut in the side of the cylinder and the slit is glazed with a stout sheet of plate-glass, the joints being made water- and steam-tight by means of india-rubber packing.

Fig. 1.



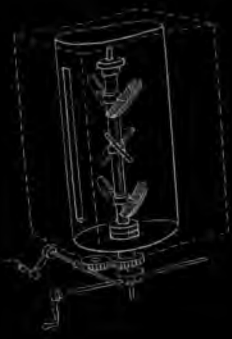


Fig. 2.

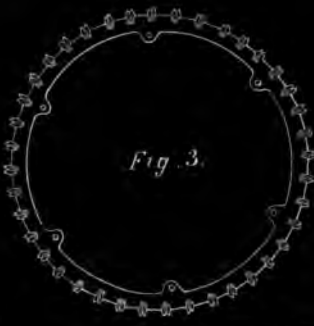
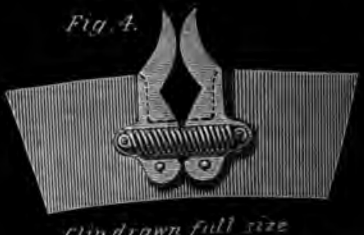


Fig. 3.

Fig. 4.



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The cylinder is placed inside a wooden box, taller than itself, and 1 ft. 5 in. square at its base, the space between it and the sides of the box being filled with sawdust, whilst the exterior of the box is completely covered with kamptulicon, in order to retain the heat of the water in the enclosed vessel as much as possible. An aperture somewhat larger than that in the cylinder is cut in the side of the box in front of it, and is also glazed with plate-glass.

A lid, containing 3 inches of sawdust, covered with a sheet of kamptulicon, can be shut tightly down on the top of the cylinder and box, the escape of the vapour given off during heating being provided for by means of a steam-pipe.

Pipes lead from the top and bottom of the water-vessel to an exterior pipe ending in a funnel above and a cock below, so that water may be poured in or drawn off from the vessel as desired.

The whole is firmly fixed to a stout wooden stand about 2 ft. high.

(2) *The Agitator* (see fig. 2).

A stout and hollow brass axis, 2¼ in. in diameter, passes vertically up through the centre of the base of the water-vessel, carrying three sets of helical vanes, one above the other, arranged so that the upper and lower vanes form segments of right-handed screws, whilst the intermediate vanes are left-handed. The inclination of every vane is adjustable.

The lower end of the axis passes through a stuffing-box in the bottom of the cylinder, and is connected by gearing to a crank-handle projecting outside the apparatus. It can be turned easily by the hand of the observer, who thereby is able to agitate the water throughout the whole depth of the vessel.

(3) *The Heating-Apparatus* (see fig. 1).

This is a copper tube 0·6 in. in diameter, which, issuing from the back of the water-vessel near the bottom, is carried through the wooden casing of the instrument, and is then coiled into a vertical spiral of six turns, gradually diminishing in diameter. The end of the tube is afterwards brought back into the water-vessel.

A cluster of Bunsen burners being placed beneath the coil serves to heat it and to make the water circulate inside the cylinder, thus warming the whole of its contents.

Experiment shows that, with the small coil used, 10 gallons of cold water can be boiled in about six hours from the time of lighting the gas; in practice, however, when it is required to test thermometers near the boiling-point only (mountain thermometers, for example) the apparatus is filled with boiling water out of kettles put on an ordinary fire.

A cone of sheet-copper is usually placed round the coil as a jacket, in order to retain the heat from the gas-burners as much as possible; this is shown by dotted lines in the drawing. A cock at the lower end of the coil permits of the stoppage of the circulation of the water through the pipe.

(4) *The Thermometer Frame.*

The thermometers to be compared are hung side by side round the circumference of two brass rings, 10¼ in. in diameter, that are attached to the side rods of a cylindrical frame. The thermometers are held in their places against the ring by spring clips, one of which is shown full size in fig. 4, and one of the rings is shown in fig. 3; the latter slide up and down the brass rods that form the sides of the cylindrical framework, and are clamped at such a distance apart as may best suit the thermometer under examination.

Forty thermometers can be suspended at a time. The bottom of the frame is provided with six rollers—three placed radially, for the purpose of guiding it up and down the interior of the water-vessel; and three tangentially and projecting below the base, in order to support the frame whilst putting on or taking off the thermometers. This operation is performed when the frame is standing on the closed lid of the box, a circular brass ring being screwed to the lid to prevent the frame running off when being turned round by the operator.

The top of the frame consists of spokes radiating from a hollow socket that drops over the end of an upright steel rod, which, passing through the axis of the agitator, projects above it (see fig. 2). A plug is then screwed into the top of this rod and clamps the frame, which is supported by it; the frame, with the thermometers, can then be rotated in the water by turning this rod. This turning is effected from the outside through a wheel fixed to its projecting lower extremity, into which an endless screw, driven by a crank in front of the apparatus, is geared. The observer, facing the glazed slit, can bring the thermometers hung round the frame before him one by one as quickly as he likes.

For the convenience of moving the thermometer frame into and out of the water-vessel, a cord is carried over the apparatus round pulleys, as seen in fig. 1, so that the attendant can hook its end to the ring at the top of the frame, and twist or lower it with the greatest facility.

The general character of the process of comparison is to turn down the gas and to close circulation in the pipe by turning the stopcock; the water is then agitated, and is afterwards left at rest until the set is finished. The thermometer frame is turned once round forwards and once backwards in each process of comparison, each instrument being read off twice, the mean of the two being the result aimed at.

Mr. Whipple, the Superintendent of the Observatory, has made at my request a large number of experiments on the variations of temperature under different conditions, and on other matters relating to the working of the apparatus. It will be sufficient if I give a few summary tables of the results.

The mean variation of temperature during a double process of comparing each of twenty sets of thermometers, each set averaging nineteen instruments, and each instrument being read four times, was as follows :—

Temperature at which the comparison was made.	50°	70°	80°	90°	100°	110°
Mean variation during each set	+0·06	±0·07	±0·06	±0·07	±0·09	±0·15

The extreme variation of 0°·30 occurred in one case, and that of 0°·25 in three cases.

It takes about four minutes to read a complete set of ordinary thermometers.

The rate of heating by gas, and of cooling after the gas has been wholly turned off, is of course much affected by the temperature of the air of the room; it may be roughly taken as follows :—

When the water in the vessel is about ...	45°	100°	150°	200°
Rise of temperature in 5 minutes when the gas is turned fully on	2°·80	2°·26	1°·95	1°·45
Fall of temperature in 5 minutes when the gas is turned wholly off............	......	0°·25	0°·60	1°·00

The rate of cooling is much reduced when the process consists in first raising the water to the highest required temperature, and then cooling it by successive additions of cold water. The heat of the stuffing that surrounds the vessel being thus much higher than the water it contains, keeps it at an equable temperature.

The temperature of the water in the vessel, after agitating it and allowing it to settle, differs somewhat at different levels; this is due to the impossibility of securing perfect intermixture and to the variations of the temperature of the stuffing in respect to that of the water. The greatest differences observed between a thermometer whose bulb was immersed 2 inches below the level of the water and one that was immersed 19 inches was 0°·68.

March 22, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Stratified Discharges.—IV. Stratified and Unstratified Forms of the Jar-Discharge." By WILLIAM SPOTTISWOODE, M.A., Treas. R.S. Received March 7, 1877.

It is well known that if a Leyden jar be discharged through a vacuum-tube, the discharge generally takes the form of an unbroken column of light, extending from the point of the positive terminal to the hilt of the negative, *i.e.* to the extreme negative end of the tube, and that it shows no trace of either negative glow or intervening dark space. On the other hand I have found, by experiments with a large Leyden battery, that if a tube have one terminal connected with the negatively charged coating of the battery and the other held beyond striking-distance from the positively charged coating, the discharge in the tube will show a separation of the positive from the negative part by a dark intervening space. Under suitable circumstances of exhaustion it will also show striæ, in the same manner as when the discharge is effected directly with a Holtz machine, having the conductors either closed or open beyond striking-distance (see Roy. Soc. Proceedings, vol. xxiii. p. 460). Again, I have found, with the same battery, that if the tube be connected otherwise as before, and held at a distance less than at first, but a little greater than striking-distance, a stratified discharge much more brilliant and more like that produced by a coil will be exhibited. It should be remarked that the latter form of discharge appears to the unassisted eye, in the cases which I have examined, as an unbroken column of light, but with a negative glow and dark space. A revolving mirror, however, resolves the column into a regular array of striæ, having a rapid proper motion towards the positive terminal.

The transition from the first to the second of these forms, and from the second to the jar-discharge proper when the tube was brought within striking-distance, was, if not absolutely abrupt, at all events so rapid that this form of experiment gave no prospect of following one form into the other. With a view to examining the transition as closely as possible a Holtz machine was employed, and the jars having been taken off, a pair of mica plates partially covered with tinfoil was used in their stead. By sliding one plate over the other, so that more or less of the covered parts were brought face to face, a jar was formed the size of which could be varied at pleasure. An air-spark of adjustable length was also introduced into the circuit between the machine and the tube.

This arrangement was subsequently replaced by the following, which in some respects proved more convenient:—A battery of one or more jars was used in the place of the mica plates. The outside of this battery and one terminal of the tube were connected with the earth; and the inside and the other terminal were alternately connected with the positive conductor of the machine, so that the battery was alternately charged and discharged through the tube. The amount of charge was regulated partly by the distance through which the conductors of the machine were separated, and partly by the number of revolutions of the machine during which the charging took place. It was consequently independent of the absolute time of contact. It will be observed that this arrangement did not give the same opportunity of a continuous variation of jar surface as the first; but, on the other hand, the changes of phase in the phenomenon due to increments of charge were capable of indefinite diminution by shortening the distance between the conductors of the machine and by increasing the number of the jars.

The first object proposed was to ascertain whether a jar could be charged with so small a quantity of electricity as of itself to give a stratified discharge in a tube; in other words, whether the resistance of the tube itself, if resistance it be, could by a suitable charge of jar be made to insure a stratified discharge. For this purpose a jar was charged with small sparks from the machine, and discharged after receiving charges of 1, 2, 3, &c. sparks in succession. The experiment proved successful with a coal-gas tube at a pressure of about 4 millims.: charges of three sparks gave bright flake-like stratifications; higher charges gave a discharge with a positive column, a negative glow, and a dark space, although the striæ were not always discernible. But when the charge exceeded 5 or 6 sparks, the positive column advanced so far as to obliterate the dark space, and ultimately made its way to the hilt of the terminal.

Similar experiments were made with both forms of instrumental arrangement, and with tubes containing different gases and at different pressures.

A number of tubes tried with various amounts of battery-charge, but with the same surface, showed that, as the charge was increased, the head of the positive column advanced towards the negative terminal, the dark space became narrower, and the glow contracted in dimensions; and when the head of the column drew very near to the negative terminal, the glow, instead of covering the whole surface of the terminal, formed a small drop at the point. On still further increasing the charge, the drop withdrew to the hilt of the terminal; and finally, when it had completely retreated into the hilt, the continuous or true jar-discharge took place.

With a view to testing experimentally how far the effects here described were due to quantity and how far to tension, the size of the jar was altered, all other circumstances remaining the same. It was then found not only, as before, that small charges gave stratified and large unstratified discharges, but also that the maximum charge compatible with stratification was greater with a large than with a small jar.

As a further experiment in this direction, a series of jars were arranged in cascade; and it was found that the greater the number of jars so arranged, the smaller the charge necessary to insure a true jar-discharge. A charge insufficient to destroy stratification with one jar was sufficient to destroy them when more than one was used in cascade. These results point to tension rather than to quantity as the determining cause of the character of the discharge.

In fact, having taken a number of jars of the same size, and having ascertained the maximum charge with which one jar could be charged without obliterating stratification, say, "the critical charge," I found that the critical charge for 2, 3, . . . jars arranged for quantity was 2, 3, . . . times that for a single jar; and, on the other hand, that the critical charge for 2, 3 . . . jars arranged in series was 1 : 2, 1 : 3, . . . of that for a single jar. The illumination, however, was always greater with the larger charges, *i. e.* with the greater quantity of electricity discharged.

The experiments above described were made first with tubes in which the pressure was moderately high. They were afterwards repeated with lower pressures, and results of the same character as before were obtained. But, owing to the smaller amount of the critical charges, to the greater extension of the negative glow, and to the consequently increased delicacy of the phenomena, the same numerical precision was not attained. But there seems no reason to doubt that the discrepancies might be indefinitely diminished by instrumental refinements.

The duration of the stratified discharges observed throughout these experiments was exceedingly short, indistinguishable, in fact, from that of the true jar-discharge. When viewed in a revolving mirror, either with or without a slit, they showed no sign whatever of prolonged duration; and we may thence conclude that, so far as our present instrumental arrangements extend, there is no inferior limit to the duration of discharge necessary for the production of striæ.

In connexion with this part of the subject another form of experiment was arranged. Beside the jars hitherto described another was used, having its inner surface connected with one terminal of the tube, and its outer with the other. When this disposition was made, the additional jar acted as a buffer, and produced a stratified discharge under circumstances which would without it have produced a true jar-discharge.

A comparison of the results here obtained with those detailed in Part II. of these researches shows that the phenomena produced by suitable disposition of the Leyden battery coincide with those produced by the induction-coil. With the coil it was found that (1) for a given electromotive force the column of striæ was shorter the larger the battery-surface or strength of current used; (2) that the proper motion, when directed, as usual, towards the positive terminal, was more rapid the greater the electromotive force employed. With the Leyden battery it was found that (1) in order to maintain the same length of column with an increased surface, the charge must be increased in a larger proportion than the surface; and (2) it was noticed that the striæ, which when the tension was low were distinct and well separated, became more blurred as the tension rose, until they sometimes were blended into an apparently unbroken column of light. The presence, however, of the negative glow still showed that the true jar-discharge had not yet been reached.

II. "On Friction between Surfaces moving at Low Speeds." By Fleeming Jenkin, F.R.SS. L. & E., Professor of Engineering in the University of Edinburgh, and J. A. Ewing. Received March 8, 1877.

(Abstract.)

The common belief regarding friction, which is based on the researches of Coulomb and Morin, is that between surfaces in motion the friction is independent of the velocity, but that the force required to start the sliding is (in some cases at least) greater than the force required to overcome friction during motion; in other words, the static coefficient is usually considered to be greater than the kinetic. It occurred to the authors that there might possibly be continuity between the two kinds of friction, instead of an abrupt change at the instant in which motion begins. We should thus expect that when the relative motion of the surfaces is very slow there will be a gradual increase of friction as the velocity diminishes. Whether any such increase takes place at very low speeds is left an open question by the experiments of Coulomb and Morin, whose methods did not enable definite measurements of the friction to be made when the velocity was exceedingly small. The authors have succeeded in measuring the friction between surfaces moving with as low a velocity as one five-thousandth of a foot per second, and have found that in certain cases there is decided increase in the coefficient of friction as the velocity diminishes.

The apparatus made use of consisted of a cast-iron disk 2 feet in diameter and weighing 86·2 lbs., supported on a steel axle whose ends were less than one tenth of an inch in diameter. These ends were supported in bearings which consisted of rectangular notches cut in pieces

of the material whose friction against steel was to be measured. The disk was caused to revolve and then left to itself, when it came to rest in consequence of the friction on the ends of the axle. The rate of retardation was found as follows :—A strip of paper 2½ inches broad was stretched round the periphery of the disk, and a pendulum was caused to swing across this paper in a plane perpendicular to that of the disk. On the pendulum was fastened a fine glass siphon, one end of which dipped into a box containing ink, whilst the other stood at a short distance from the paper strip, across which it was carried as the pendulum oscillated. By keeping the ink-box strongly electrified ink was deposited on the paper by the point of the siphon in a rapid succession of fine spots. By this means, without the introduction of any new source of friction, a permanent record was made of the resultant motion of the pendulum and the revolving disk. This frictionless method of recording was designed by Sir William Thomson for telegraphic purposes, and is employed in his siphon recorder. From the curve drawn in this way it was easy to determine the rate of retardation of the disk (and therefore the friction) corresponding to various velocities of the rubbing surfaces. The lowest velocity for which the determinations were definite was about 0·0002 foot per second, and the highest velocity to which the experiments extended was 0·01 foot per second. The surfaces examined were steel on steel, steel on brass, steel on agate, steel on beech, and steel on greenheart—in each case under the three conditions, dry, oiled, and wet with water. In the cases steel on beech oiled or wet with water, and steel on greenheart oiled or wet with water, the coefficient of friction increased as the velocity diminished between the two limits given above, the increase amounting to about twenty per cent. of the lower value. It appeared that at the higher limit of velocity there was little further tendency to change in the coefficient; but it is impossible to say how much additional change might take place between the lower limit of the velocity and rest. In the case of steel on agate wet with water there was a similar but much less marked increase of friction as the velocity decreased; and in the case of steel on steel oiled there was a slight and somewhat uncertain change of the opposite character—that is, a decrease of friction as the velocity decreased. This case, however, would require further examination. In all other cases the friction seemed to be perfectly constant and independent of the velocity. Out of all the sets of circumstances investigated, the only ones in which there was a large difference between the static and kinetic values of the coefficient of friction were those in which a decided increase was observed in the kinetic value as the speed decreased. This result renders it exceedingly probable that there is continuity between the two kinds of friction.

III. "Magnetic Observations at Kerguelen." By the Rev. S. J. Perry, S.J., F.R.S. Received March 15, 1877.

The Government expedition to Kerguelen Island for the observation of the Transit of Venus on December 8th, 1874, presented a very favourable opportunity for the accurate determination of the magnetic elements of an important station in the South-Indian Ocean, and one which will at most be visited only at distant intervals for the purpose of scientific investigations. When, therefore, I heard of my appointment to that station, I at once brought the question of terrestrial magnetism under the notice of the Astronomer Royal, and he readily agreed to my proposal of taking a complete set of magnetic instruments to Kerguelen, and of making any observations that would not interfere with the main object of our expedition.

Being in charge of the whole Kerguelen party of observers, I could not expect to be able to devote much time personally to the magnetic work; but the Rev. W. Sidgreaves, whom long experience, both at the Stonyhurst Observatory and during our magnetic survey of France in 1868 and 1869, had made perfectly conversant with all the details both of instruments and observations connected with terrestrial magnetism, had already been placed on the staff of astronomical observers for Kerguelen. The assistance of a very efficient observer being thus secured, the next step was to procure the necessary instruments. Fortunately I experienced no difficulty in this matter, as the authorities at Kew immediately placed at my disposal a Jones unifilar and a Barrow dip-circle. There was no question of the want of a good chronometer, as the astronomers were to be supplied with nine of these, besides the eight reserved exclusively for longitude connexions, which remained always undisturbed in their quiet berth on shipboard.

It was at first proposed by Sir Edward Sabine that we should take a series of magnetic observations at sea during our voyage from England to the Cape of Good Hope and thence to Kerguelen, and a special instrument was ready for the purpose; but as it was finally arranged that we should perform the first part of our journey in the mail-steamer, it was thought advisable by the Hydrographer of the Admiralty to relinquish all idea of taking magnetic observations at sea.

The land instruments were made use of on almost every available occasion, both at Kerguelen and during our journey; but I will confine myself in this paper to the Kerguelen results, reserving the other observations for a separate communication.

The constants for the temperature-correction, and for other data regarding the magnets employed, were kindly determined for me by Mr. Whipple at Kew.

No correction for error of graduation of deflection-bar was found necessary.

The angular value of one division of the scale of the vibration-magn No. 3 $=2'\cdot3$. This was again tested at the Cape of Good Hope, ar found to be $2'\ 16''\cdot7$. The same magnet had for its induction-coefficie 0·0002204, and its dimensions of inertia were the following:—Leng 0·31736 foot, diameter 0·032681 foot, and weight 979·127 grains.

The correction to 35° Fahr. was

$$0\cdot0001583\ (t-35^\circ)+0\cdot000000465\ (t-35^\circ)^2;$$

and log π^2k at 60° Fahr. $=1\cdot675300$.

Our chief astronomical station at Kerguelen was Observatory Bay, little to the south of the north-west corner of Royal Sound, its approx mate latitude being 49° 25′ 11″·9 S., and its longitude $4^h\ 39^m\ 34^s\cdot3$ of Greenwich. At this station a long series of observations of the di horizontal force, and declination were taken during our four month stay. The trips undertaken for the establishment of our two secondar stations at Swain's Haulover and at Thumb Peak, and also for the long tude connexions, afforded opportunities of observing the magnetic elemen at the second and third British stations, as well as at that of the America at Molloy Point. The existence of a magnetic observatory at the Germa station rendered unnecessary any further observations at Betsy Cove. brick pier was erected on a solid foundation at Observatory Bay for th magnetic instruments, and most of the observations were made on thi spot.

The Magnetic Dip.

The dip observations were taken with three needles, and the result obtained on different days are all entered in the following Table:—

	1874–75.	No. 1.	No. 2.	No. 3.	Mean.
		° ′ ″	° ′ ″	° ′ ″	° ′ ″
Observatory Bay ...	November 13...	......	72 4 1	72 5 15	72 4 38
Swain's Haulover ...	December 13...	70 59 58	71 3 2	70 57 10	71 0 3
Thumb Peak	December 21...	......	71 6 38	......	71 6 38
Observatory Bay .	December 23...	......	71 57 6	71 54 30	71 55 48
	January 13 ...	......	71 52 40	71 56 54	71 54 47
	January 30 ...	71 58 30	71 54 21	72 3 34	71 58 48
	February 18 ...	......	71 42 6	......	71 42 6

The station at Swain's Haulover was six or seven miles to the sout of Observatory Bay, and Thumb Peak about the same distance due eas of the Haulover. At Thumb Peak there was only time to observe wit a single needle; the weather, too, was bad, and the spot chosen not ver favourable, being on the shingle near the water's edge. The readings o January 30th were taken on the rocks near the landing cove. The las observation at Observatory Bay was made near the top of the rock over-hanging the dwelling, and in a rather unsteady position; it is therefor less reliable than the others. The true dip for January 1st, 1875, a Observatory Bay will probably be a little in excess of 71° 55′ 13″·4 which is the mean of the observed values.

The Magnetic Intensity.

The horizontal component of the intensity was determined in the usual way by observations of vibration and deflection. Only one set of observations was taken at a distance from the chief station, viz. that on December 13th, at Swain's Haulover.

Vibration Observations.

Station.	1874–75.	Temperature.	Time of one vibration.	Log *m* X.	Value of *m*.
Observatory Bay	November 13...	44°·4	5·47797	0·19703	0·45548
	„ 24...	59·6	5·48405	0·19724	0·45559
	December 10...	50·7	5·48567	0·19618	0·45476
Swain's Haulover...	„ 13...	49·9	5·57271	0·18250	0·45450
Observatory Bay	January 6	55·7	5·49071	0·19605	0·45372
	„ 16	47·9	5·48535	0·19621	0·45527
	„ 25	48·8	5·48425	0·19650	0·45569
	February 9 ...	58·5	5·49062	0·19629	0·45546

Deflection Observations.

Station.	1874–75.	Distances of centres of magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$.
		ft.		° ′ ″	
Observatory Bay	November 13...	1·0	38°·3	15 11 50	9·12003
		1·3	38·3	6 52 1	9·11980
	December 10...	1·0	42·7	15 9 16	9·11920
		1·3	43·2	6 51 23	9·11955
Swain's Haulover...	„ 13...	1·0	44·9	15 38 14	9·13267
		1·3	45·7	7 3 39	9·13246
Observatory Bay	January 6	1·0	52·2	15 3 59	9·11755
		1·3	50·0	6 48 53	9·11750
	„ 16	1·0	46·5	15 10 49	9·12032
	„ 29	1·0	41·5	15 13 0	9·12084
	February 9......	1·0	51·3	15 10 41	9·12060

In the above Tables *m* represents the magnetic moment of the vibration-needle, and X the earth's horizontal magnetic intensity.

In deducing the vertical component and the total intensity from the horizontal force, I have made use of the dip obtained on the same day as the vibrations and deflections, or on the nearest day possible. As, however, the observation of February 18th is far from reliable, I have adopted for February 9th the mean value of the dip deduced from the December and January observations taken at Observatory Bay, viz. 71° 56′ 28″.

Station.	H. F.	V. F.	T. F.
Observatory Bay	3·4559	10·6852	11·2327
	3·4567	10·6876	11·2353
	3·4547	10·5884	11·1378
Swain's Haulover	3·3494	9·7278	10·2883
Observatory Bay	3·4615	10·5986	11·1496
	3·4510	10·5665	11·1158
	3·4501	10·5952	11·1428
	3·4502	10·5816	11·1299

If, now, we combine each determination of the horizontal force with the mean value of the dip for December and January, we obtain the following results at Observatory Bay for the Vertical Force and the Total Intensity:—

V. F.	T. F.
10·5991	11·1483
10·6015	11·1509
10·5954	11·1444
10·6163	11·1663
10·5841	11·1325
10·5813	11·1296
10·5816	11·1299

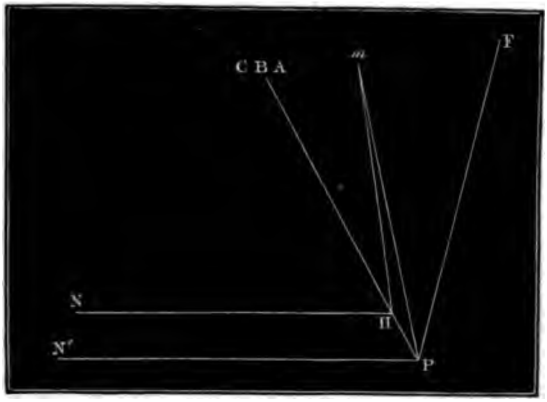
The probable error of the mean value of the intensity is thus reduced from ±0·0126 to ±0·0035; we may therefore consider 11·1431 as the adopted value of the total force for January 1st, 1875.

The Magnetic Declination.

The advantage of a fixed observatory enabled us to determine with great exactness the direction of the astronomical meridian, and to connect the position of the magnetic pier with the sites of the astronomical instruments. On January 28th and on February 5th observations of the sun were taken with an excellent transit theodolite made expressly for the expedition by Messrs. Troughton and Simms. The results give the following values for the azimuth of a well-defined point on the Prince of Wales's Foreland, some ten miles distant:—

	Circle right.	Circle left.
	° ′ ″	° ′ ″
January 28th	95 17 10	95 18 27
February 5th	95 19 30·5	95 18 18
	95 18 20·25	95 18 22·5

The resulting azimuth of the Foreland would therefore be 95° 18′ 21″·38 E. of the north point. The correctness of this angle, on which all the declinations depend, was tested by a series of measurements connecting the azimuth of the Foreland (F) with the azimuth mark (*m*) of the great theodolite, a most accurate instrument, especially designed by Sir G. B. Airy for the lunar longitude observations at Kerguelen. As the Foreland was not visible from the altazimuth hut (H), which stood, however, not far from the magnetic pier (P), the two marks were connected as follows:—Let N, N′ be the true north, and A, B, C distant points on a mountain seen over H from P. By careful readings of A, B, C, and *m* from P and H the following values of the angle *m* were found—7′ 18″, 7′ 26″, 7′ 20″, 7′ 37″, 7′ 28″, 7′ 14″, giving as a mean 7′ 25″. This, applied to the azimuth of *m* from H (which was accurately known from daily readings to be 55° 41′ 12″), made the azimuth of *m* from P =55° 33′ 47″. The angle *m* P F was then found by repeated measures to be 39° 44′ 22″, which gave as a final result the angle F P N =95° 18′ 9″. As this is almost identical with the value obtained above from direct solar observations, we can adopt the mean of the two, viz. 95° 18′ 15″, as the azimuth of the Foreland; and thus we have only to take the bearing of the magnet with respect to this point in order to obtain the results included in the subjoined Table.



Observed Declinations.

1874–75.	L.M.T.	Circle reading of Magnet.	Scale correction.	Circle reading of Foreland.	Bearing of magnet south of Foreland.	W. Declination.	Daily means.
	h m	° ′ ″	′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
Nov. 13 …	3 50 P.M.	175 43 55	−32 21	126 13 55	48 57 39	35 44 6	35 44 6
Nov. 24 …	10 40 A.M.	182 34 20	,, ,,	133 12 50	49 9	52 36	52 36
	10 50	,, ,,	,, ,,	,, ,,	,, ,,	,, ,,	
Dec. 3……	6 0 P.M.	236 44 15	,, ,,	187 19 35	51 19	50 26	50 26
Dec. 10 …	4 0	233 44 15	,, ,,	184 20 20	51 34	50 11	50 11
Jan. 6……	4 5	193 45 5	,, ,,	144 16 0	56 44	45 1	45 1
	4 40	,, ,,	,, ,,	,, ,,	,, ,,	,, ,,	
Jan. 13 …	12 0	260 23 50	,, ,,	210 58 50	52 39	49 6	48 8
	2 15 P.M.	,, ,,	−31 54	,, ,,	53 6	48 39	
	3 15	,, ,,	−30 45	,, ,,	54 15	47 30	
	3 40	,, ,,	−29 37	,, ,,	55 23	46 22	
	4 15	,, ,,	−29 10	,, ,,	55 50	45 55	

Observed Declinations (*continued*).

1874-75.	L.M.T.	Circle reading of Magnet.	Scale correction.	Circle reading of Foreland.	Bearing of magnet south of Foreland.	W. Declination.	Daily means.
	h m	° ′ ″	′ ″	° ′ ″	° ′ ″	° ′ ″	
Jan. 13 ...	4 35 P.M.	260 23 50	−32 35	210 58 50	48 52 25	35 49 20	Moved magnet before observation.
	5 15	" "	−32 21	" "	52 39	49 6	
	6 15	" "	" "	" "	" "	" "	
Jan. 16 ...	12 15	195 25 40	" "	146 3 20	49 59	51 46	35 51 39
	12 45	" "	−32 7	" "	50 13	51 32	
Jan. 21 ...	10 30 A.M.	241 27 20	−32 21	192 3 25	51 34	50 11	47 54
	11 15	" "	−27 20	" "	56 35	45 10	
	11 45	" "	−25 58	" "	57 57	43 48	
	12 15 P.M.	" "	−27 20	" "	56 35	45 10	
	12 45	" "	−26 26	" "	57 29	44 16	
	4 25	144 52 10	−32 21	95 30 0	49 49	51 56	
	5 0	" "	−32 7	" "	50 3	51 42	
	6 30	" "	−31 26	" "	50 44	51 1	
Jan. 25 ...	4 42	214 12 40	−32 21	164 46 10	54 9	47 36	48 10
	5 5	" "	−33 29	" "	53 1	48 44	
Feb. 6......	10 0 A.M.	192 51 15	−32 21	143 31 45	57 9	44 36	43 43
	10 35	" "	−28 56	" "	49 0 34	41 11	
	11 10	" "	−28 1	" "	1 29	40 16	
	11 40	" "	−27 20	" "	2 10	39 35	
	2 45 P.M.	193 2 0	−31 54	143 27 0	2 12	39 33	New suspension-thread.
	4 15	193 24 0	−32 21	143 59 10	48 52 29	49 16	
	6 15	" "	−34 38	" "	50 12	51 33	
Feb. 8......	10 0 A.M.	254 25 0	−32 35	205 5 10	47 15	54 30	35 49 2
	10 30	" "	−32 21	" "	47 29	54 16	
	11 0	" "	−31 54	" "	47 56	53 49	
	11 30	" "	" "	" "	" "	" "	
	12 0	" "	−31 26	" "	48 24	53 21	
	12 30 P.M.	" "	−28 29	" "	51 21	50 24	Magnet vibrating considerably.
	1 30	" "	−25 58	" "	53 52	47 53	
	2 30	" "	−22 20	" "	57 30	44 15	
	3 0	" "	−20 44	" "	59 6	42 39	
	3 50	" "	−20 17	" "	59 33	42 12	
	5 30	" "	−23 28	" "	56 22	45 23	
	6 0	" "	−24 23	" "	55 27	46 18	
	6 45	" "	−26 26	" "	53 24	48 21	
	7 0	" "	−27 20	" "	52 30	49 15	
Feb. 9......	3 0	182 53 40	−32 21	133 24 5	57 14	44 31	35 43 55
	3 30	" "	−29 37	" "	59 58	41 47	
	4 0	" "	−33 16	" "	56 19	45 26	
Feb. 11...	9 45 A.M.	185 48 40	−32 21	136 30 10	46 9	55 36	54 18
	10 0	" "	−34 24	" "	44 6	57 39	
	10 30	" "	−33 57	" "	44 33	57 12	Magnet oscillating violently.
	11 0	" "	−32 21	" "	46 9	55 36	
	11 30	" "	−33 16	" "	47 4	54 41	
	12 15 P.M.	" "	−38 2	" "	51 50	49 55	
	12 30	" "	−38 31	" "	52 19	49 26	

Adopted mean .... 35° 48′ 24″ W.

The declination magnet used throughout these observations was incapable of rapid reversion; and therefore the zero of the scale was tested carefully by several reversions previous to our departure from the island, and it was found to be 53·9. A very favourable series of reversions at Cape-Town Observatory had previously given 54·2 as the zero division.

The above readings of the declination show an absolute maximum of 35° 57′ 12″ at 10 A.M. on February 11th, and a minimum of 35° 39′ 33″ at 2.45 P.M. on February 6th; the range, therefore, for the month, as far as observed, is 17′ 39″, and the mean declination =35° 48′ 22″·5. The daily means give a slightly larger W. declination, with a range of only 10′ 12″ for the whole series of observations.

Nothing more of course than the very roughest idea can be formed of the diurnal range from the few observations taken; but there is some evidence at least of an easterly movement of the needle between 10 A.M. and 3 P.M., followed by a westerly motion that continued till 7 P.M. The greatest mean velocity of the magnet was about 3′ 40″ per hour at 2 P.M.; but the velocity once reached 8′ an hour, viz. at 10 A.M., during the disturbance on February 11th. As there are many disturbing causes that probably affect the earth's magnetism as a whole, it may not be irrelevant to remark that an examination of the Stonyhurst magnetograms on all the days occurring in the above Table of Declinations shows that February 11th was the only disturbed day in England, and that, with the exception of a slight tremulous motion of the needle on November 13th and 14th, on December 10th, and on February 8th, the observing days were remarkably quiet.

Besides the series of observations taken at Observatory Bay, other determinations of the declination were made at Swain's Haulover, at Thumb Peak, and at Molloy Point; but as some doubt still remains to be cleared up respecting the errors and rates of the chronometers employed in the sun observations, I will defer the publication of the results until this essential point is satisfactorily settled.

Previous to these observations, taken in connexion with the Government Transit-of-Venus Expedition, the only magnetic observations at Kerguelen on record, if we except any possible results obtained by Captain Cook in the last century, are those of Sir J. Ross in 1840 and of H.M.S. 'Challenger' in 1874. The values contained in Sir Edward Sabine's "Contributions to Terrestrial Magnestism, No. XI." (Phil. Trans. 1868), furnish the data necessary for a comparison with our present work.

Adopting 71° 56′ 28″ for the dip of the south end of the needle at Observatory Bay on January 1st, 1875, and comparing this value with the observation of Sir J. Ross in 1840, after correcting the dip of −70°·0 at Christmas Harbour by −0°·4 for change of station to Royal Sound, we find a secular variation of about −2′·7. But if we consult the Table of numerical coefficients deduced from all the collected observations, and take the value of the dip in 1840 for the station whose longitude is 70° E.

and latitude 50° S., which was approximately our position at Observatory Bay, we obtain a secular variation of $-2'\cdot3$. We may therefore fairly conclude that $-2'\cdot5$ represents the annual change with considerable accuracy.

Passing from the dip to the total force we find 11·323 to be in British units the mean of three determinations from observations made on shore by H.M.S. 'Erebus' and 'Terror.' If, now, we apply the correction +0·1 for the change from Christmas Harbour to Royal Sound, the result is still somewhat less than the mean of the observations taken near the eastern extremity of Kerguelen during the epoch 1840–45. Adopting 11·423 as the mean value for 1842–45, and 11·143 for 1875, we obtain a secular diminution of 0·0086 in this element of terrestrial magnetism.

The annual increase of the declination will be $+7'\cdot0$, if we take the approximate value of 32·0 W. from the map of Sir E. Sabine as representing the declination for the epoch 1842–45.

IV. "On the Variations of the Daily Range of the Magnetic Declination as recorded at the Kew Observatory." By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at the Owens College, Manchester. Received February 28, 1877.

1. The daily range of the magnetic declination at any station may perhaps be regarded as a convenient representative of the magnetic activity of the place. For while a thorough discussion of the diurnal magnetic changes must embrace along with the declination the two components of the force, yet, as regards such daily ranges, the declination gives results which are not only more prominent but also more easily procurable and subject to fewer uncertainties than similar ones for the other two elements.

In estimating the daily range of the magnetic declination, as recorded at the Kew Observatory, I have excluded the disturbed observations, conceiving that by so doing a better indication of the true magnetical activity of the place would be obtained than by including them, inasmuch as they follow a very different set of laws from that of the well-known diurnal declination-range. The disturbed observations have been separated by the method of Sir E. Sabine, those being rejected as disturbed for which the measurements on the photographic curve are 0·150 inch either above or below the mean value for that month and hour, one inch denoting $22'\cdot04$ of angular change. The daily ranges are here given in inches, and they denote the differences between the greatest and least values of each day's hourly tabulations from the curve, disturbances being excluded. I am indebted to the kindness of the Kew Committee for giving me the daily ranges herein discussed, extending from the beginning of 1858 to the end of 1873, thus embracing in all sixteen years' observations.

A. *Annual Variation of Declination-range.*

2. The following Table exhibits mean monthly results of the declination-range corresponding to 48 points in the year. It will afterwards be seen (art. 7) that the declination-range depends amongst other things on the relative position of the sun and moon, and hence to eliminate this inequality I have resorted to monthly means.

TABLE I.—Containing Monthly Means (48 to the year) of the Diurnal Declination-ranges, thus:—January (0) gives the Monthly Mean of which the Middle Date is the very commencement of the Year, January (1) that for one Week after the commencement, and so on.

Date.		1858–61.	1862–65.	1866–9.	1870–3.	Mean.
Jan.	(0)	·325	·320	·249	·352	·312
„	(1)	·334	·329	·265	·367	·323
„	(2)	·344	·348	·279	·389	·340
„	(3)	·356	·363	·313	·414	·362
Feb.	(0)	·389	·369	·347	·435	·385
„	(1)	·414	·371	·359	·458	·401
„	(2)	·438	·379	·378	·476	·418
„	(3)	·479	·389	·388	·496	·438
Mar.	(0)	·512	·418	·395	·545	·467
„	(1)	·554	·465	·425	·589	·508
„	(2)	·593	·504	·463	·634	·548
„	(3)	·635	·538	·499	·675	·587
April	(0)	·664	·554	·537	·704	·615
„	(1)	·689	·552	·556	·731	·632
„	(2)	·697	·547	·555	·755	·639
„	(3)	·664	·535	·545	·738	·620
May	(0)	·641	·526	·516	·713	·599
„	(1)	·605	·528	·504	·688	·581
„	(2)	·600	·532	·508	·652	·573
„	(3)	·619	·549	·516	·657	·586
June	(0)	·626	·568	·529	·663	·596
„	(1)	·637	·574	·538	·669	·605
„	(2)	·633	·582	·541	·685	·610
„	(3)	·614	·581	·539	·683	·604
July	(0)	·613	·566	·533	·692	·601
„	(1)	·606	·558	·533	·692	·597
„	(2)	·611	·547	·526	·678	·591
„	(3)	·612	·537	·528	·692	·593
Aug.	(0)	·611	·546	·538	·681	·594
„	(1)	·623	·551	·544	·684	·601
„	(2)	·635	·558	·550	·700	·611
„	(3)	·631	·562	·544	·686	·606
Sept.	(0)	·623	·547	·534	·671	·594

TABLE I. (*continued*).

Date.		1858–61.	1862–65.	1866–9.	1870–3.	Mean.
Sept.	(1)	·609	·540	·514	·646	·577
„	(2)	·581	·523	·494	·621	·554
„	(3)	·559	·493	·481	·595	·532
Oct.	(0)	·537	·483	·458	·573	·513
„	(1)	·522	·464	·445	·552	·496
„	(2)	·504	·448	·437	·522	·478
„	(3)	·486	·445	·418	·503	·463
Nov.	(0)	·465	·427	·408	·480	·445
„	(1)	·420	·402	·389	·462	·418
„	(2)	·389	·376	·361	·430	·389
„	(3)	·363	·354	·333	·390	·360
Dec.	(0)	·341	·337	·309	·371	·340
„	(1)	·341	·321	·279	·345	·322
„	(2)	·323	·311	·259	·339	·308
„	(3)	·325	·305	·254	·349	·308

3. It will be seen from Table I. that while there is a maximum of declination-range in June about the time of the summer solstice, there are also maxima in April and August, and that a behaviour of this kind is indicated in each four years' observations. Comparing this result with that embodying the annual variation of temperature-range at Kew (Proc. Roy. Soc. 1877, vol. xxv. p. 578), it will be seen that the latter variation has only one maximum in July. Perhaps there is a reference to the equinoxes as well as to the solstices in the annual variation of the declination-range. A comparison of the two is exhibited in Figs. IX., X., p. 120 (Fig. IX. giving declination- and Fig. X. temperature-ranges).

B. *Variations of Long Period.*

4. It is well known that the range of the magnetic declination has a long-period variation, apparently connected with the physical state of the sun's surface. In order to investigate the nature and closeness of this connexion the following plan has been adopted:—Let us assume as the most probable hypothesis that the cause which exalts or depresses the mean annual declination-range exalts or depresses also in a similar manner the variations of this from one month to another. This is what would take place if we could imagine the effect to be produced by some influence *emanating* from the sun, which acted more powerfully on some years than on others, while the variations of this effect due to the sun's position in the ecliptic were also altered in the same proportion. On the whole this is borne out by Table I. Constructing, now, a Table for each year, and for 48 points in each year, and reckoning the mean of the 16 years' ranges for each of these points (as exhibited in the last column of Table I.) equal to 1000, we find in Table II. a series of values exhibiting the proportion between the observed range for any point of any one year,

and the mean of the whole 16 years for the same point. For instance, the monthly value corresponding to Feb. (0) 1866 is ·3535, while the normal value for the whole 16 years for this point is (by Table I.) ·385, and hence the proportional value of the range for Feb. (0) 1866 is $1000 \times \frac{\cdot 3535}{\cdot 385} = 918$. By these means it is believed that the results of Table II. are freed from any recognized inequality, depending either on the month of the year or on the relative position of the sun and moon.

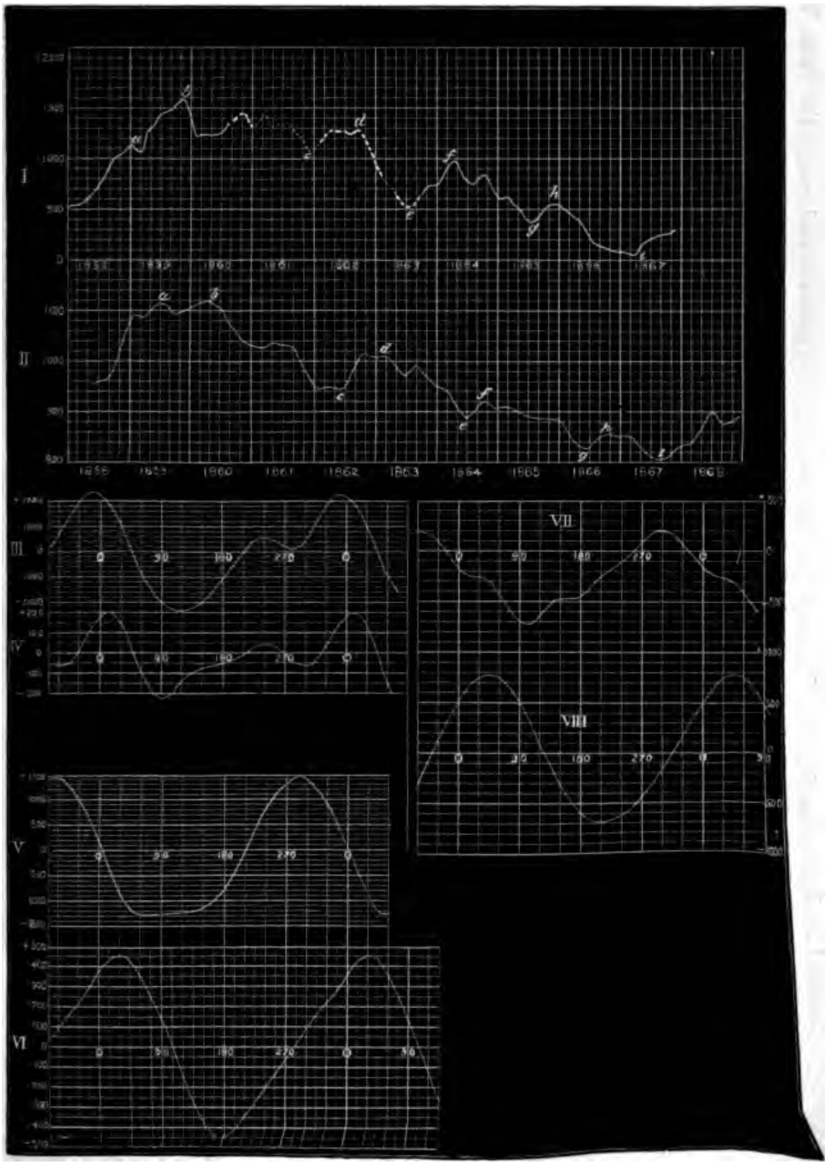


TABLE II.—Exhibiting Monthly Means of Declination-range (48 points to each year), the Mean Value of the Range for the whole Series for each point being reckoned = 1000.

		1858.	1859.	1860.	1861.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	1871.	1872.	1873.
January	(0).........	—	1070	1140	970	1015	1127	979	991	887	921	709	676	913	1135	1155	1217
,,	(1).........	—	1018	1132	944	977	1120	1007	985	920	923	764	683	972	1142	1238	1203
,,	(2).........	913	986	1134	1017	956	1146	980	1011	925	852	793	710	1006	1089	1270	1212
,,	(3).........	924	1007	1011	1002	960	1143	903	1014	942	870	830	818	1011	1094	1280	1193
February	(0).........	966	1006	1044	1028	942	1048	921	923	918	871	881	931	1091	1059	1249	1122
,,	(1).........	1001	1061	1016	1053	935	1031	832	909	901	866	830	991	1143	1080	1203	1147
,,	(2).........	1010	1108	1037	1032	873	1003	805	948	888	882	836	1017	1154	1159	1149	1098
,,	(3).........	1034	1139	1140	1065	848	966	844	892	873	873	844	954	1156	1169	1094	1109
March	(0).........	1022	1147	1172	1040	825	928	876	949	816	836	813	915	1161	1256	1099	1145
,,	(1).........	1025	1149	1175	1013	830	948	943	936	793	817	845	891	1167	1256	1106	1103
,,	(2).........	1025	1161	1168	968	885	949	946	897	781	827	852	915	1173	1219	1104	1129
,,	(3).........	988	1185	1147	1011	907	936	924	898	806	791	867	938	1138	1216	1145	1102
April	(0).........	952	1228	1084	1059	881	972	876	879	847	765	932	946	1143	1198	1114	1126
,,	(1).........	940	1253	1090	1081	870	980	821	821	876	770	933	938	1162	1221	1109	1135
,,	(2).........	910	1259	1057	1138	855	958	801	810	841	752	957	930	1197	1284	1123	1128
,,	(3).........	889	1223	1056	1112	830	974	830	814	821	797	936	960	1230	1285	1134	1110
May	(0).........	896	1180	1101	1102	869	983	836	825	791	779	906	969	1262	1279	1175	1047
,,	(1).........	836	1135	1124	1068	869	1003	869	892	787	791	895	993	1261	1290	1178	1009
,,	(2).........	880	1091	1150	1063	906	1024	900	887	862	792	847	1046	1256	1186	1124	986
,,	(3).........	878	1113	1190	1052	951	1022	894	886	843	767	850	1064	1222	1211	1097	959
June	(0).........	886	1089	1184	1041	972	1024	917	894	875	773	844	1055	1180	1225	1096	947
,,	(1).........	902	1051	1189	1072	994	992	933	880	847	770	872	1072	1171	1187	1115	954
,,	(2).........	845	1078	1180	1047	1030	972	916	895	812	786	868	1081	1190	1221	1153	926

July	(0)………	892	1083	1079	1027	1007	985	931	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	948
”	(1)………	911	1071	1059	1016	1016	947	920	856	810	815	849	1095	1318	1203	1168	947
”	(2)………	982	1042	1122	994	981	954	928	843	796	810	870	1085	1266	1194	1144	990
”	(3)………	949	1037	1141	1004	982	938	884	828	794	798	881	1093	1286	1203	1156	1026
August	(0)………	930	1007	1168	1008	983	951	891	850	803	812	919	1090	1254	1209	1131	994
”	(1)………	919	1058	1164	1011	983	926	888	874	751	838	971	1064	1206	1258	1088	1001
”	(2)………	891	1104	1115	1050	985	912	883	872	770	852	956	1024	1228	1268	1111	977
”	(3)………	922	1084	1101	1062	998	914	892	904	774	867	929	1025	1218	1239	1075	998
September	(0)………	956	1118	1048	1072	985	930	870	903	778	853	908	1057	1230	1244	1041	1007
”	(1)………	980	1129	1031	1081	1001	952	877	915	805	824	870	1060	1236	1163	1071	1004
”	(2)………	1014	1109	1027	1040	985	974	864	947	789	808	894	1071	1277	1118	1061	1021
”	(3)………	1069	1148	1005	981	959	961	839	948	819	801	929	1067	1237	1137	1098	1002
October	(0)………	1052	1108	1074	954	960	951	871	984	834	788	918	1035	1225	1143	1117	985
”	(1)………	1097	1102	1072	938	937	963	885	960	835	795	948	1014	1228	1180	1073	974
”	(2)………	1124	1100	1070	925	953	946	916	936	839	820	971	1029	1170	1205	1070	926
”	(3)………	1096	1072	1085	944	984	1001	928	934	866	813	927	1004	1233	1174	1036	903
November	(0)………	1074	1094	1046	967	996	1018	920	905	890	822	918	1036	1220	1137	1047	907
”	(1)………	1025	1060	1000	934	1026	1021	890	907	934	811	915	1058	1294	1125	1099	902
”	(2)………	1003	1007	988	1002	1038	1052	868	909	1000	827	885	997	1277	1137	1109	903
”	(3)………	939	1094	963	1039	1081	1076	872	900	981	795	906	1016	1204	1117	1150	865
December	(0)………	966	1090	922	1042	1057	1095	937	881	955	791	908	986	1195	1172	1117	885
”	(1)………	1018	1147	999	1082	1041	1120	976	860	948	777	813	929	1069	1236	1115	871
”	(2)………	1010	1187	938	1057	1039	1147	1000	850	946	727	745	946	1124	1263	1149	874
”	(3)………	1054	1185	966	1008	1016	1081	1040	821	899	749	684	971	1164	1299	1140	

5. The numbers of Table II. require to be further dealt with before they can be made to furnish a curve, bringing out the long-period variation of the declination-range. Let us first take for this purpose, as well as for other objects to be afterwards mentioned, a series of values derived from the numbers of Table II., each representing the mean of 12 consecutive values of Table II. These may be termed three-monthly values. Thus, for instance, we have as follows:—

TABLE III.—Exhibiting the Method of obtaining Three-monthly Values.

Date, 1858.	Monthly Values for Table II.	Three-Monthly Values.	
Feb. (3)	1034		
		 983	
Mar. (0)	1022		.. 983
		 983	
„ (1)	1025		
		 980	
„ (2)	1025		.. 977
		 974	
„ (3)	988		
		 961	
Apr. (0)	952		.. 955
		 950	
„ (1)	940		

We have thus, in the last column of Table III., a series of three-monthly values corresponding to the beginning and middle points of each month. In the next place, by adding together a certain three of these values, we may obtain nine-monthly values. Thus the three-monthly value for March (0), as above, is 983, while that for June (0) is 885, and for Sept. (0) 986; the mean of these (being 951) is the nine-monthly value corresponding to June (0). Nine-monthly values have thus been obtained corresponding to the beginnings of each month; and finally, by adding these together, two and two, a series of nine-monthly values have been obtained corresponding to the middle points of each month. These are given in Table IV., and a curve exhibiting them is likewise given in Fig. II. attached to this paper. Again, the numbers given by Messrs. De La Rue, Stewart, and Loewy in their paper on "Solar Physics" (Phil. Trans. 1870, page 111), exhibiting the spotted area of the sun's visible hemisphere for the years for which we have Kew declination results, have been treated in a manner precisely similar to the above; that is to say, nine-monthly values corresponding to the middle of each month have been obtained. These values are given in Table V., and a curve exhibiting them is likewise given in Fig. I. (p. 105).

TABLE IV.—Declination-range, Nine-monthly Values.

	1858.	1859.	1860.	1861.	1862.	1863.	1864.	1865.	1866.	1867.	1868.
Jan. (2)	...	1082	1109	1030	945	1008	945	909	879	851	832
Feb. (2)	...	1090	1113	1029	942	1009	942	910	867	850	837
Mar. (2)	...	1088	1116	1026	947	1006	934	907	850	844	846
April (2)	...	1094	1117	1030	946	996	916	902	837	832	859
May (2)	...	1105	1109	1036	941	983	895	898	829	820	876
June (2)	957	1112	1104	1032	942	971	887	894	826	811	891
July (2)	960	1112	1093	1029	956	976	890	890	832	804	898
Aug. (2)	962	1107	1075	1027	979	988	900	889	844	799	887
Sept. (2)	975	1095	1063	1016	1002	986	914	888	851	801	874
Oct. (2)	997	1092	1050	995	1013	974	921	888	854	807	878
Nov. (2)	1030	1097	1040	975	1010	962	918	888	852	816	885
Dec. (2)	1061	1102	1034	960	1007	952	911	884	851	827	

TABLE V.—Spotted Areas, Nine-monthly Values.

	1858.	1859.	1860.	1861.	1862.	1863.	1864.	1865.	1866.	1867.
Jan. (2)	504	1122	1311	1343	1112	913	770	598	522	72
Feb. (2)	530	1086	1220	1400	1173	829	868	605	482	65
Mar. (2)	538	1107	1246	1426	1249	745	943	574	438	55
April (2)	595	1241	1240	1359	1266	698	982	510	410	86
May (2)	654	1316	1244	1313	1268	623	904	474	361	153
June (2)	706	1361	1254	1333	1285	560	803	415	283	194
July (2)	778	1446	1292	1352	1249	515	766	366	198	211
Aug. (2)	871	1462	1357	1316	1271	528	760	398	144	234
Sept. (2)	983	1485	1370	1265	1294	606	823	461	120	251
Oct. (2)	1030	1532	1402	1236	1231	671	830	513	100	262
Nov. (2)	1051	1563	1437	1150	1133	710	736	535	85	305
Dec. (2)	1100	1500	1378	1077	1005	715	643	537	78	

6. If we compare together Figs. I. and II. (p. 105), it will be seen that there are a good many points in the one curve which we are fairly entitled to identify with corresponding points in the other; of these, *b* and *i* represent the respective maximum and minimum points. There is, however, a fluctuation between *d* and *e* on the declination-curve that has no corresponding fluctuation on the sun-spot curve; while, on the other hand, there are a series of small fluctuations on the sun-spot curve between *b* and *c* which have no distinct analogues on the declination-curve. It will, however, be seen that both of these discordant regions are represented by dotted lines on the sun-spot curve; that is to say, they represent results derived either wholly or in part from Schwabe's eye-observations while the Kew photo-heliograph was not in action.

Again, it will be remarked that each of the corresponding points occurs later in point of time in the declination than in the sun-spot curve. Thus we have :—

	Date in Solar curve.	Date in Declination-curve.	Difference, in Months.
a.	Jan. 15, 1859	July 15, 1859	6
b.	Nov. 15, 1859	Apr. 15, 1860	5
c.	Dec. 15, 1861	June 0, 1862	5½ doubtful.
d.	Sept. 15, 1862	Mar. 0, 1863	5½
e.	Aug. 0, 1863	June 15, 1864	10½ doubtful.
f.	Apr. 15, 1864	Oct. 15, 1864	6
g.	July 15, 1865	June 0, 1866	10½
h.	Dec. 15, 1865	Oct. 15, 1866	10
i.	Mar. 15, 1867	Aug. 15, 1867	5

I shall return again to this subject at a future part of this paper.

C. *Lunar Annual Variation.*

7. For the purpose of discovering this variation the whole period of observation has been portioned out into lunations, beginning with new moon. Each lunation is divided into eight parts, entitled (0), (1), (2), (3), (4), (5), (6), (7); (0) denoting new, and (4) full moon.

These various lunations thus divided, with the corresponding values of the declination-range, are exhibited in Table VI., the values of which have been obtained by a method of treatment precisely similar to that adopted for the Kew temperature-ranges, and described in a previous paper (Proc. Roy. Soc. 1877, vol. xxv. p. 581).

TABLE VI.—Exhibiting the Declination-ranges grouped according to Lunations.

Running No.	Lunation commencing new moon.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1.	Jan. 15, 1858.	·287	·322	·296	·261	·317	·437	·417	·387
2.	Feb. 13, „	·504	·470	·383	·383	·473	·557	·519	·504
3.	Mar. 15, „	·565	·609	·591	·531	·550	·622	·628	·529
4.	Apr. 13, „	·558	·606	·636	·557	·542	·465	·520	·510
5.	May 13, „	·488	·412	·446	·561	·578	·544	·542	·487
6.	June 11, „	·561	·546	·536	·439	·426	·558	·563	·536
7.	July 10, „	·572	·616	·539	·568	·617	·615	·534	·462
8.	Aug. 9, „	·537	·570	·556	·541	·552	·582	·575	·500
9.	Sept. 7, „	·526	·633	·595	·531	·486	·537	·613	·602
10.	Oct. 7, „	·506	·499	·508	·539	·580	·524	·522	·480
11.	Nov. 5, „	·415	·362	·417	·404	·337	·382	·375	·276
12.	Dec. 5, „	·220	·289	·367	·367	·351	·374	·319	·286
13.	Jan. 4, 1859.	·289	·348	·364	·366	·387	·294	·293	·368
14.	Feb. 3, „	·387	·482	·471	·426	·479	·500	·493	·511
15.	Mar. 4, „	·555	·569	·624	·645	·664	·704	·676	·742
16.	Apr. 3, „	·742	·746	·867	·914	·894	·819	·766	·711
17.	May 2, „	·603	·613	·621	·655	·670	·640	·570	·622
18.	June 1, „	·736	·710	·667	·625	·607	·576	·647	·694
19.	June 30, „	·740	·662	·561	·656	·681	·537	·537	·599
20.	July 29, „	·646	·685	·599	·586	·638	·652	·697	·739
21.	Aug. 28, „	·753	·616	·547	·652	·646	·670	·631	·611

TABLE VI. (*continued*).

Running No.	Lunation commencing new moon.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
22.	Sept. 26, 1859.	·603	·621	·558	·520	·472	·503	·548	·532
23.	Oct. 26, „	·549	·475	·413	·451	·464	·433	·392	·370
24.	Nov. 24, „	·340	·332	·388	·412	·386	·378	·345	·366
25.	Dec. 24, „	·317	·317	·402	·450	·365	·315	·347	·398
26.	Jan. 23, 1860.	·442	·403	·359	·342	·382	·467	·435	·445
27.	Feb. 21, „	·458	·461	·533	·590	·633	·709	·662	·594
28.	Mar. 22, „	·662	·661	·617	·720	·720	·643	·719	·716
29.	Apr. 21, „	·684	·625	·598	·597	·677	·660	·639	·688
30.	May 20, „	·687	·663	·624	·659	·788	·822	·690	·686
31.	June 19, „	·738	·684	·629	·573	·634	·652	·594	·546
32.	July 18, „	·617	·760	·772	·786	·738	·677	·613	·648
33.	Aug. 16, „	·700	·701	·668	·700	·697	·614	·492	·486
34.	Sept. 15, „	·504	·568	·620	·573	·521	·470	·518	·539
35.	Oct. 14, „	·586	·527	·483	·522	·509	·460	·446	·419
36.	Nov. 13, „	·400	·367	·319	·323	·380	·359	·272	·293
37.	Dec. 12, „	·274	·303	·353	·318	·244	·243	·321	·295
38.	Jan. 11, 1861.	·300	·297	·361	·395	·417	·390	·395	·362
39.	Feb. 9, „	·417	·418	·466	·511	·448	·452	·518	·511
40.	Mar. 11, „	·524	·511	·576	·468	·485	·670	·760	·781
41.	Apr. 10, „	·765	·703	·712	·709	·714	·683	·627	·639
42.	May 9, „	·638	·596	·557	·551	·586	·622	·632	·659
43.	June 8, „	·655	·634	·638	·668	·690	·584	·565	·617
44.	July 8, „	·637	·597	·563	·621	·604	·555	·559	·593
45.	Aug. 6, „	·684	·631	·565	·624	·671	·718	·644	·615
46.	Sept. 4, „	·653	·628	·569	·601	·599	·538	·449	·392
47.	Oct. 4, „	·436	·450	·425	·478	·526	·466	·394	·388
48.	Nov. 2, „	·374	·410	·439	·406	·405	·374	·386	·347
49.	Dec. 2, „	·354	·371	·343	·318	·330	·318	·281	·288
50,	Dec. 31, „	·335	·302	·252	·374	·345	·308	·328	·319
51,	Jan. 30, 1862.	·358	·370	·394	·408	·374	·374	·343	·275
52,	Feb. 28, „	·314	·412	·478	·473	·484	·512	·500	·525
53.	Mar. 30, „	·553	·588	·539	·480	·577	·548	·501	·484
54.	Apr. 28, „	·552	·498	·511	·522	·533	·497	·477	·553
55.	May 28, „	·579	·626	·622	·587	·631	·583	·568	·629
56.	June 27, „	·692	·637	·562	·558	·578	·635	·610	·537
57.	July 26, „	·576	·582	·557	·567	·558	·623	·623	·604
58.	Aug. 25, „	·646	·635	·588	·582	·522	·527	·519	·570
59.	Sept. 23, „	·578	·522	·450	·442	·407	·446	·492	·448
60.	Oct. 23, „	·445	·483	·460	·414	·448	·394	·395	·422
61.	Nov. 21, „	·390	·383	·377	·382	·370	·297	·292	·273
62.	Dec. 21, „	·305	·337	·314	·300	·388	·438	·429	·404
63.	Jan. 19, 1863.	·347	·321	·413	·434	·423	·454	·400	·345
64.	Feb. 18, „	·385	·430	·443	·446	·453	·459	·445	·497
65.	Mar. 19, „	·566	·600	·589	·608	·580	·552	·625	·654
66.	Apr. 18, „	·678	·630	·573	·547	·521	·580	·586	·615
67.	May 17, „	·663	·621	·612	·572	·606	·581	·566	·629
68.	June 16, „	·634	·595	·538	·575	·599	·610	·617	·573
69.	July 15, „	·549	·533	·492	·551	·590	·577	·584	·590
70.	Aug. 14, „	·580	·454	·474	·538	·578	·561	·569	·581
71.	Sept. 13, „	·590	·538	·502	·515	·497	·487	·448	·451
72.	Oct. 12, „	·497	·469	·467	·480	·418	·455	·478	·457
73.	Nov. 11, „	·443	·411	·376	·340	·321	·355	·418	·430
74.	Dec. 10, „	·422	·340	·314	·341	·298	·327	·319	·317
75.	Jan. 9, 1864.	·265	·278	·374	·380	·332	·358	·323	·300
76.	Feb. 7, „	·325	·371	·382	·297	·293	·360	·406	·427
77.	Mar. 8, „	·528	·550	·534	·576	·589	·516	·477	·516

TABLE VI. (*continued*).

Running No.	Lunation commencing new moon.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
78.	Apr. 6, 1864.	·587	·557	·500	·468	·518	·478	·507	·536
79.	May 6, „	·523	·491	·469	·479	·504	·548	·520	·571
80.	June 4, „	·572	·598	·563	·557	·584	·556	·505	·540
81.	July 4, „	·628	·583	·523	·510	·553	·522	·486	·494
82.	Aug. 2, „	·497	·544	·526	·567	·600	·548	·520	·546
83.	Sept. 1, „	·531	·542	·471	·433	·452	·519	·456	·407
84.	Sept. 30, „	·439	·406	·436	·422	·443	·483	·490	·441
85.	Oct. 30, „	·403	·369	·346	·368	·385	·370	·296	·275
86.	Nov. 29, „	·269	·286	·311	·341	·366	·315	·325	·309
87.	Dec. 28, „	·247	·212	·329	·387	·326	·303	·357	·335
88.	Jan. 27, 1865.	·364	·348	·384	·424	·314	·358	·390	·425
89.	Feb. 25, „	·484	·470	·400	·408	·513	·531	·515	·550
90.	Mar. 27, „	·559	·533	·531	·564	·561	·476	·414	·485
91.	Apr. 25, „	·563	·556	·509	·442	·523	·539	·526	·521
92.	May 24, „	·512	·493	·492	·535	·559	·588	·565	·499
93.	June 23, „	·543	·552	·539	·514	·479	·495	·478	·489
94.	July 22, „	·530	·504	·492	·473	·542	·553	·539	·567
95.	Aug. 21, „	·568	·509	·529	·502	·557	·548	·500	·520
96.	Sept. 19, „	·528	·513	·516	·480	·463	·486	·496	·454
97.	Oct. 19, „	·423	·443	·409	·387	·376	·440	·393	·356
98.	Nov. 18, „	·303	·346	·310	·295	·305	·285	·277	·224
99.	Dec. 18, „	·230	·244	·243	·274	·263	·234	·317	·376
100.	Jan. 16, 1866.	·329	·308	·315	·332	·313	·333	·378	·426
101.	Feb. 15, „	·399	·346	·349	·372	·359	·399	·471	·410
102.	Mar. 16, „	·395	·415	·460	·450	·528	·580	·579	·614
103.	Apr. 15, „	·638	·569	·490	·396	·437	·498	·382	·435
104.	May 14, „	·516	·538	·507	·511	·515	·474	·482	·547
105.	June 12, „	·606	·560	·455	·442	·505	·463	·429	·434
106.	July 12, „	·498	·543	·519	·477	·445	·444	·438	·488
107.	Aug. 10, „	·503	·473	·427	·449	·489	·479	·480	·452
108.	Sept. 9, „	·477	·453	·440	·402	·428	·416	·372	·447
109.	Oct. 8, „	·472	·460	·445	·365	·334	·332	·353	·442
110.	Nov. 7, „	·448	·427	·389	·349	·296	·309	·388	·314
111.	Dec. 7, „	·296	·305	·324	·319	·219	·233	·289	·217
112.	Jan. 6, 1867.	·309	·349	·358	·343	·288	·238	·241	·294
113.	Feb. 4, „	·346	·419	·442	·395	·356	·311	·358	·397
114.	Mar. 6, „	·400	·477	·500	·443	·447	·466	·487	·496
115.	Apr. 4, „	·446	·395	·483	·534	·547	·477	·472	·515
116.	May 4, „	·503	·429	·385	·400	·514	·443	·384	·503
117.	June 2, „	·508	·468	·396	·469	·521	·507	·509	·465
118.	July 1, „	·508	·512	·430	·473	·481	·500	·455	·457
119.	July 31, „	·504	·486	·470	·519	·547	·583	·550	·528
120.	Aug. 29, „	·543	·493	·484	·431	·453	·485	·420	·401
121.	Sept. 27, „	·424	·431	·387	·348	·339	·418	·426	·410
122.	Oct. 27, „	·408	·349	·341	·298	·309	·361	·304	·314
123.	Nov. 26, „	·358	·247	[·212]	[·237]	·233	·208	·237	·222
124.	Dec. 26, „	·224	·245	·231	·242	[·254]	[·266]	·277	·323
125.	Jan. 24, 1868.	·361	·319	·293	·369	·376	·348	·318	·336
126.	Feb. 23, „	·426	·391	·392	·421	·419	·398	·464	·504
127.	Mar. 24, „	·556	·538	·545	·588	·640	·646	·626	·550
128.	Apr. 22, „	·580	·636	·557	·487	·502	·507	·489	·473
129.	May 22, „	·495	·524	·468	·450	·553	·536	·516	·558
130.	June 20, „	·563	·515	·509	·512	·539	·508	·497	·487
131.	July 19, „	·493	·548	·516	·532	·573	·594	·584	·616
132.	Aug. 18, „	·671	·603	·548	·511	·480	·512	·530	·488
133.	Sept. 16, „	·452	·480	·533	·504	·463	·493	·442	·384

TABLE VI. (*continued*).

Running No.	Lunation commencing new moon.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
134.	Oct. 15, 1868.	·437	·498	·474	·491	·391	·353	·384	·310
135.	Nov. 14, „	·295	·351	·384	·353	·253	·239	·258	·276
136.	Dec. 14, „	·240	·213	·186	·205	·200	·181	·197	·275
137.	Jan. 12, 1869.	·258	·212	·239	·215	·283	·327	·417	·492
138.	Feb. 11, „	·501	·417	·400	·412	·399	·351	·419	·521
139.	Mar. 13, „	·527	·467	·476	·586	·586	·560	·578	·611
140.	Apr. 12, „	·666	·591	·538	·530	·582	·609	·584	·616
141.	May 11, „	·623	·588	·521	·509	·624	·689	·682	·711
142.	June 10, „	·602	·561	·601	·653	·704	·713	·684	·655
143.	July 9, „	·612	·593	·619	·643	·690	·679	·661	·668
144.	Aug. 7, „	·656	·601	·591	·619	·646	·635	·593	·668
145.	Sept. 6, „	·667	·640	·622	·565	·550	·565	·496	·529
146.	Oct. 5, „	·575	·522	·477	·436	·441	·504	·496	·475
147.	Nov. 3, „	·439	·443	·475	·392	·359	·378	·304	·258
148.	Dec. 3, „	·320	·367	·339	·311	·234	·218	·245	·290
149.	Jan. 2, 1870.	·344	·316	·294	·269	·284	·345	·380	·374
150.	Jan. 31, „	·414	·475	·518	·488	·461	·500	·483	·453
151.	Mar. 2, „	·535	·592	·644	·649	·651	·709	·690	·659
152.	Apr. 1, „	·742	·704	·811	·775	·741	·811	·790	·786
153.	Apr. 30, „	·745	·665	·714	·753	·761	·702	·692	·738
154.	May 30, „	·732	·692	·619	·643	·759	·806	·715	·751
155.	June 28, „	·840	·742	·709	·826	·823	·852	·790	·695
156.	July 28, „	·659	·696	·776	·745	·722	·799	·719	·681
157.	Aug. 26, „	·739	·766	·750	·713	·720	·729	·637	·652
158.	Sept. 25, „	·721	·704	·614	·547	·570	·589	·601	·562
159.	Oct. 24, „	·470	·586	·611	·571	·509	·528	·590	·559
160.	Nov. 23, „	·418	·375	·325	·335	·343	·390	·363	·312
161.	Dec. 22, „	·339	·335	·373	·361	·367	·360	·372	·357
162.	Jan. 21, 1871.	·372	·359	·378	·461	·471	·442	·419	·495
163.	Feb. 19, „	·489	·557	·582	·582	·603	·682	·735	·712
164.	Mar. 21, „	·679	·680	·673	·690	·812	·823	·797	·758
165.	Apr. 19, „	·819	·852	·887	·814	·671	·629	·650	·779
166.	May 19, „	·747	·600	·583	·717	·793	·855	·773	·750
167.	June 18, „	·699	·635	·716	·751	·762	·673	·677	·738
168.	July 17, „	·748	·634	·589	·704	·767	·761	·722	·737
169.	Aug. 16, „	·841	·829	·797	·748	·702	·684	·713	·663
170.	Sept. 14, „	·679	·678	·495	·476	·583	·626	·638	·625
171.	Oct. 14, „	·625	·617	·559	·489	·504	·512	·449	·421
172.	Nov. 12, „	·478	·493	·432	·419	·396	·333	·359	·434
173.	Dec. 12, „	·445	·449	·422	·396	·318	·364	·358	·412
174.	Jan. 10, 1872.	·392	·431	·478	·475	·496	·504	·484	·478
175.	Feb. 9, „	·482	·508	·484	·446	·478	·474	·467	·501
176.	Mar. 9, „	·584	·628	·628	·671	·664	·632	·728	·741
177.	Apr. 8, „	·733	·704	·668	·724	·763	·732	·625	·678
178.	May 7, „	·719	·679	·671	·604	·611	·621	·590	·610
179.	June 6, „	·723	·753	·671	·692	·759	·704	·671	·678
180.	July 5, „	·679	·744	·735	·731	·684	·608	·588	·649
181.	Aug. 4, „	·728	·729	·684	·615	·646	·621	·639	·686
182.	Sept. 3, „	·629	·609	·568	·560	·608	·609	·572	·561
183.	Oct. 2, „	·591	·608	·524	·466	·428	·455	·483	·489
184.	Nov. 1, „	·507	·459	·459	·440	·432	·432	·391	·393
185.	Nov. 30, „	·411	·405	·338	·302	·329	·349	·365	·347
186.	Dec. 30, „	·355	·413	·376	·377	·419	·411	·386	·459
187.	Jan. 28, 1873.	·447	·467	·476	·413	·407	·446	·494	·456
188.	Feb. 27, „	·520	·571	·532	·580	·597	·583	·623	·712
189.	Mar. 28, „	·706	·658	·693	·795	·791	·694	·710	·729

TABLE VI. (*continued*).

Running No.	Lunation commencing new moon.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
190.	Apr. 26, 1873.	·733	·599	·568	·547	·548	·575	·516	·559
191.	May 26, ,,	·627	·616	·560	·519	·547	·568	·593	·585
192.	June 24, ,,	·567	·556	·529	·530	·625	·628	·524	·562
193.	July 24, ,,	·649	·651	·566	·622	·612	·575	·575	·602
194.	Aug. 23, ,,	·599	·614	·627	·608	·606	·578	·539	·520
195.	Sept. 21, ,,	·570	·578	·534	·513	·477	·478	·424	·393
196.	Oct. 21, ,,	·465	·417	·411	·411	·383	·336	·385	·349
197.	Nov. 20, ,,	·315	·375	·323	·236	·223	[·243]	[·263]	·282

8. Making use of the whole series of lunations of Table VI. we obtain the following results:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range......	·519	·512	·499	·499	·507	·508	·499	·503	(A)

a series which presents the appearance of a double period with maxima about new and full moon. A similar result has been obtained for Lisbon by Senhor Capello, Director of the observatory there ('Annals of the Observatory,' 1876), who finds that the declination-ranges, or rather the differences of the declination at 8 A.M. and at 2 P.M., obey a law similar to that stated above.

It may likewise be remarked (as was done in the corresponding discussion of temperate-ranges) that the sum of the four left-hand numbers is larger than that of the four right-hand numbers—the former being 2·029, while the latter is 2·017.

D. *Semiannual Lunar Variation.*

9. If we now make use of the lunations corresponding to the six months of which the middle point is the winter solstice, employing for this purpose lunations 1–2, 9–15, 22–27, 34–39, 47–52, 59–64, 71–76, 84–89, 96–101, 108–114, 121–126, 133–138, 146–151, 158–163, 170–175, 183–188, 195–197 (in all 97 lunations) we obtain the following result:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of winter range............	·415	·420	·415	·408	·401	·409	·413	·412	(B)

But before making use of these numbers we must apply to them a small correction. For it is possible that the sum of the various new-moon observations for any six winter months, inasmuch as they occur at dates preceding those of the corresponding full-moon observations, or observations for other phases, may be affected differently from the latter by the annual variation indicated in Table I. A correction on this account has therefore been obtained from Table I., and when applied to (B) we obtain the following result:—

Phase of lunation...	(0)	(1)	(2)	(3	(4)	(5)	(6)	(7)	
Corrected value of winter range......	·417	·422	·416	·409	·402	·409	·411	·408	(C)

Series (C) is represented in Fig. XI. (p. 120).

·10. If we now make use of the observations corresponding to the six months grouped around the summer solstice (100 in all), we obtain the following results:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of summer range...........	·621	·601	·580	·587	·610	·604	·582	·591	(D)

and if we apply to this a residual correction analogous to that applied to (B), we obtain as follows:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Corrected value of summer range.	·620	·600	·578	·586	·609	·604	·584	·596	(E)

In series (E) we have well-marked maxima corresponding to new and to full moon.

E. *Variations which seem to depend on Planetary Configurations.*

11. From art. 6 we may conclude that the connexion between solar spotted areas and declination-ranges is an intimate one. Now Messrs. De La Rue, Stewart, and Loewy, in a paper already quoted (Phil. Trans. 1870), have shown that the amount of spotted area of the sun's surface exhibits a reference to the chief planetary configurations. It becomes, therefore, a question of interest to ask whether declination-ranges exhibit a reference of the same kind[*].

In order to reply to this I have selected those configurations which occur most frequently, and which might therefore be supposed to be sufficiently well indicated by sixteen years' observations.

These are, (α) the period of conjunction of Venus and Mercury, (β) the solar period of Mercury, (γ) the period of conjunction of Venus and Jupiter.

In the next place, three-monthly values for every week have been constructed after the manner indicated in Table III. Now inasmuch as the periods of the three configurations already alluded to are not very far different from three months, we may imagine that these three-monthly values are to a great extent free from any inequality depending on these periods. The differences between the monthly and the three-monthly values will, however, exhibit any such inequality as may exist. These differences, slightly equalized, are therefore made to form the ordinates of a curve of which the time is the abscissa, and we may expect to derive from such a curve materials for determining whether there be any inequality in the declination-range due to such configurations. The method employed in plotting this curve will be understood from the following example:—

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[*] Mr. C. Chambers, of the Bombay Observatory, has discussed the question as to whether certain other magnetic elements have a reference of this kind (Phil. Trans. 1875, p. 361).

TABLE VII.

Date, 1858.	Monthly value.	Three-monthly value.	Difference.	Final equalized difference, plotted in the curve.
Feb. (3)	1034			
		 983	.... +45	
Mar. (0)	1022			+43
		 983	.... +40	
„ (1)	1025			+42
		 980	.... +45	
„ (2)	1025			+38
		 974	.... +32	
„ (3)	988			+21
		 961	.... + 9	
April (0)	952			+ 2
		 950	.... − 4	
„ (1)	940			

12. With regard to the first configuration mentioned (the period of conjunction of Venus and Mercury), these observations embrace 39 periods in all; and summing up the ordinates of the curve corresponding to each 30 degrees of angular separation for the various 39 periods, precisely after the manner employed in the paper on Solar Physics already referred to, we obtain the following result:—

TABLE VIII.—Venus and Mercury together (0° denotes conjunction).

Between	0°	and	30°	+193
„	30	„	60	+ 23
„	60	„	90	−196
„	90	„	120	−207
„	120	„	150	− 93
„	150	„	180	− 59
„	180	„	210	− 43
„	210	„	240	+ 13
„	240	„	270	+ 26
„	270	„	300	− 52
„	300	„	330	− 49
„	330	„	360	119

In Figs. III. and IV. (p. 105) the sun-spot and the declination-curve for this configuration are exhibited together. It will be noticed that there is a very striking likeness between the two, the declination-curve, however, lagging behind the other in point of time, as might be expected from art. 6.

13. Next with regard to the second configuration (the solar period of Mercury), the results are so decided that half the declination observations are sufficient to give a tolerably good value. This will be seen from the following Table :—

TABLE IX.—Period of Mercury about the Sun (in all 65 sets: 0° denotes Perihelion).

				First half.	Second half.	Whole series.
Between	0°	and	30°	+217	+212	+429
„	30	„	60	+153	+280	+433
„	60	„	90	− 3	+259	+256
„	90	„	120	−168	+173	+ 5
„	120	„	150	−281	+ 1	−280
„	150	„	180	−276	−163	−439
„	180	„	210	−151	−262	−413
„	210	„	240	− 5	−274	−279
„	240	„	270	+ 73	−213	−140
„	270	„	300	+114	−101	+ 13
„	300	„	330	+145	+ 13	+158
„	330	„	360	+181	+ 97	+278

In Figs. V. and VI. the supposed inequalities due to this period are compared together for spotted solar area and declination-range. It will be observed that the latter lags visibly behind the former in point of time.

14. Let us, in the last place, consider the period of the conjunction of Jupiter and Mercury. In this case, as in the previous one, the inequality is so well marked that the observations may be split into two series; this will be seen from the following Table :—

TABLE X.—Period of Conjunction of Mercury and Jupiter (in all 63 sets: 0° denotes conjunction).

				First half.	Second half.	Whole series.
Between	0°	and	30°	+198	+435	+633
„	30	„	60	+236	+523	+759
„	60	„	90	+225	+427	+652
„	90	„	120	+119	+209	+328
„	120	„	150	− 46	− 73	−119
„	150	„	180	−185	−319	−504
„	180	„	210	−251	−427	−678
„	210	„	240	−230	−447	−677
„	240	„	270	−157	−391	−548
„	270	„	300	− 91	−231	−322
„	300	„	330	0	− 10	− 10
„	330	„	360	+118	+225	+343

In Figs. VII. and VIII. the supposed inequalities due to the above period are compared together for solar spotted area and declination-range. It will be noticed that the latter lags visibly behind the former in point of time.

F. *Remarks on the supposed relations between Solar spotted areas, Declination-ranges, and Temperature-ranges.*

15. A few remarks on this subject will not be considered unallowable if the object be not so much to introduce a final theory as to suggest a working hypothesis which, while not inconsistent with any well-established fact, may perhaps serve to direct future inquiry.

In the first place, we may conclude, as the result of the comparison of Figs. I. and II., that the connexion between spotted areas and declination-ranges is of an intimate nature, the smaller inequalities of the one being reproduced in the other with modifications.

16. In the next place, it seems almost certain that sun-spots are not the chief cause of magnetic action. Mr. Broun, in a recent paper "On the Decennial Period in the Range and Disturbance of the Diurnal Oscillations of the Magnetic Needle and in Sun-spot area" (Trans. Roy. Soc. Edinb. 1876), has made a remark similar to the above, founding it upon the fact that sun-spots appear only when the magnetic action exceeds a given value.

17. Nevertheless it is most probable that magnetic activity is somehow caused by the sun, depending perhaps on the physical state of his surface, while sun-spots give us only a rough mode of estimating this physical state, just as rainfall might in estimating the climate of a place. For it will be seen that the effect of the sun upon magnetic range bears all the appearances of being due to an influence *emanating* from our luminary. For just as the maxima of yearly and daily temperature lag behind the corresponding maxima of solar heat influence, so do the maxima and minima of declination-range lag behind the corresponding maxima and minima in the solar curve, while the same lagging behind appears in the curves, denoting the supposed influence of the planets on the state of the solar surface and (through it?) on the magnetic range.

18. Again, we may probably imagine that sun-spots give us a roughly true indication of solar activity; for if this were not so it would be difficult to account for the striking likeness between the sun-spot planetary curves and the declination-range planetary curves. That the sun-spots afford but a rough indication of the physical state of the sun will of course be gathered from the fact that the sun is influential both in meteorology and magnetism when there are no spots; and the same conclusion appears to be supported by the fact that the planetary inequalities appear to be more pronounced when derived from declination-ranges than when derived from sun-spots.

19. There seems, however, to be something more than this; there

appears to be in the march of the declination-range from year to year (Fig. II.) traces of a force which prevents this range from being strictly comparable with that of sun-spots. It will be seen that after the date of peculiarity *a* (Figs. I. and II.) the sun-spot curve marches rapidly up, while the declination-range curve does not so mount; also, after the maximum *b*, the sun-spot curve falls more rapidly than the declination-curve. Similar remarks will apply to other points; in fine we have grounds for supposing the declination-range to be acted upon by some other influence than one so represented by sun-spots as to follow their increase and diminution.

Mr. J. A. Broun, in a series of interesting investigations, has indicated the probability that there is an influence of this nature; and it may fairly be said that the results of this paper are at least consistent with such an hypothesis.

20. I would next remark that the hypothesis asserting a connexion of some kind between magnetical and meteorological phenomena appears to be borne out by the results of this paper*.

It will be noticed from Figs. XI., XII. (p. 120), that there is a striking likeness between the winter lunar variation for the declination and temperature ranges. There is also a likeness between the summer lunar variation for these two elements, not so striking to the eye, but which will nevertheless be seen from the following comparison:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Summer lunar variation temperature-range	16·96	17·02	17·23	17·22	17·35	17·15	17·24	17·27
Summer lunar variation declination-range	·620	·600	·578	·586	·609	·604	·584	·596

Both of these, the first imperfectly and the latter fully, exhibit maxima at or near new and full moon. Again, while on the whole there is a likeness between the curves representing the annual variation for these two elements, yet there is also a dissimilarity, inasmuch as the declination-curve (Fig. IX.) has apparently a strong reference to the equinoxes, which is absent, or nearly so, in the temperature-curve. But it may be taken for granted that if there be a connexion between magnetism and meteorology, it certainly cannot be of such a nature that all the meteorological peculiarities of a place are reproduced in its magnetic phenomena, for all observation is against a connexion of this description. Indeed any hypothesis of a connexion between these two must, in order to be consistent with facts, assume that the magnet averages things so as to be free, in a great measure if not completely, from local peculiarities.

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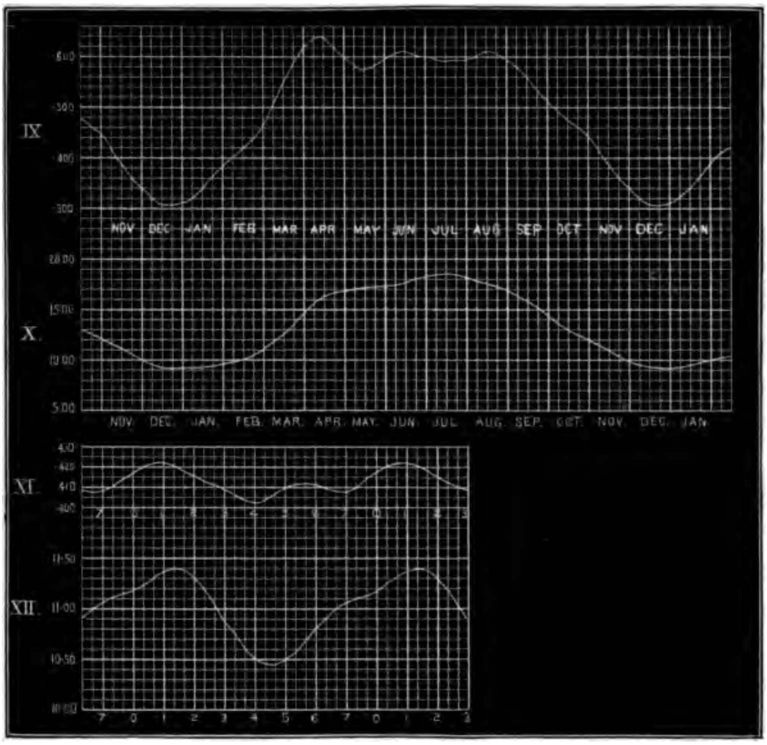
* Mr. Baxendell, of Manchester, was the first to direct attention to this subject in a paper "On a Diurnal Inequality in the Direction and Velocity of the Wind," apparently connected with the daily changes of magnetic declination. See Memoirs of the Lit. and Phil. Society of Manchester, vol. iv. ser. 3, p. 210.

The results of this paper appear to be consistent with such an hypothesis when so modified.



21. It is needless here to enter into the various reasons which induce us to believe in the existence of a connexion between the meteorology of the earth and the physical state of the sun's surface. I may, however, refer to a paper "On the Daily Range of Atmospheric Temperature at the Kew Observatory" (Proc. Roy. Soc. 1877, vol. xxv. p. 580), in which it was shown that at Kew the temperature-range is somewhat higher at times of maximum than at times of minimum sun-spots. If, however, we plot as a curve this temperature-range, it is neither like Fig. I. nor Fig. II., or at least not so like as to suggest any marked relation to the eye. (This curve is not given in this paper.) But on examining its most prominent points, I find that not a few of these agree both in direction and in time with similar peculiarities in the magnetic curve. Thus there is a well-marked prominence in the temperature-range curve corresponding to about the end of May 1861; now there is a prominence in the magnetic curve at about the same date. Again, there is a depression in both curves corresponding to about the end of May 1862. Again, there is a well-marked depression in the temperature-curve corresponding to the end of April 1866, while in the

declination-curve there is a well-marked depression perhaps a month later. Finally, there is a depression in the temperature-curve corresponding to the beginning of July 1867, and one in the declination-curve corresponding to the middle of August. I have not been able to notice any marked coincidence between the temperature-range and the sun-spot curves.

Without attempting to decide the question, it appears that there is at least some preliminary evidence in favour of an alliance between the three phenomena, solar spotted area, terrestrial meteorology, and terrestrial magnetism, of such a nature that the variations of the former precede those of the other two in point of time. It will be seen that this is a question of much importance; for if there be a connexion of this nature, once its laws are known, it may become possible to foresee the character of impending meteorological changes. These points, however, can only be determined by further investigations.

I desire, before concluding, to thank Mr. Wm. Dodgson, who has given me much assistance in the calculations and diagrams of this paper.

The Society then adjourned over the Easter Recess, to Thursday, April 12.

Presents received, March 1, 1877.

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April 12, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On certain Molecular Changes which occur in Iron and Steel during the separate acts of Heating and Cooling." By Professor Norris, M.D., Queen's College, Birmingham. Communicated by G. Gore, F.R.S. Received August 15, 1876.

(Abstract.)

In the course of certain researches in Physiological Physics, in which the author of this paper has been for some time past engaged, it became necessary, with a view to learn how far it bore upon the subject in hand, to inquire into the nature of a peculiar fact which was observed by his friend Mr. Gore, and published in the Proceedings of the Royal Society, January 28th, 1869, under the title of "A Momentary Molecular Change in Iron Wire." In this communication it was shown that if a strained iron wire were heated to redness by a voltaic current or other means, on being allowed to cool, the contraction of cooling was at a certain point, and for a limited period, arrested by an action of elongation.

"With wires of iron 0·65 millim. thick (No. 23) and 21·5 centims. long, strained to the extent of ten ounces or more, and heated to full redness, the phenomenon was clearly developed. For example, the needle of the instrument went with regularity to 18·5 of index-plate; the current was then stopped; the needle instantly retreated to 17·75, then as quickly advanced to 19·75, and then went slowly and regularly back, but not to zero. After shutting off the source of heat, the wire contracted 0·75 millim., and then expanded 2 millims.; so that it returned 1·25 millim.

beyond the original point of heating, and then resumed its course towards zero, which it failed to reach. The length of the kick, plus the amount of stretch, occasioned during the occurrence by the strain was therefore 2 millims. If the temperature of the wire was not sufficiently high, or the strain upon the wire not enough, the needle went directly back without exhibiting the momentary forward movement. The temperature and strain required to be sufficient to actually stretch the wire somewhat at the higher temperature. A higher temperature with a less degree of strain, or a greater degree of strain with a somewhat lower temperature, did not develop the phenomenon. The wire was found to be permanently elongated on cooling."

Mr. Gore further observes:—"The molecular change evidently includes a diminution of cohesion at a particular temperature during the process of cooling; and it is interesting to notice that at the same temperature during the heating-process no such loss of cohesion (nor any increase of cohesion) takes place; a certain temperature and strain are therefore not alone sufficient to produce it; the condition of cooling must also be included. The phenomena which occur during cooling are not the exact converse of those which take place during heating."

The subject was subsequently taken up by Professor Barrett, of Dublin, who, in a paper to the 'Philosophical Magazine' in 1873 (vol. xlvi. p. 472), showed, by the use of the mirror, that during the heating of the wire a converse action to that which takes place during cooling occurs. He says:—"During the heating of the wire a slight and momentary retrogression of the beam was noticed at the temperature corresponding to the powerful jerk that occurred on cooling.

"The temperature at which the momentary jerk occurs seems to be lower in thick wires than in thin ones. The momentary *retraction*, as closely as can be judged, takes place at the same temperature at which the *elongation* takes place on cooling. Releasing the tension of the spring, the forward motion on cooling is, as might be expected, much lessened, whilst the jerk back is scarcely affected. Increasing the tension of the spring, the forward jerk is correspondingly increased, and the backward jerk diminishes and can be made to disappear. Without the spring, an iron wire can be seen by the naked eye to undergo a momentary contraction during heating, and a momentary and more palpable elongation during cooling.

"All kinds of iron do not exhibit this behaviour; and some show it in a more or less marked degree. I have not been able to detect any change in certain specimens of good soft iron wire; but in hard iron wire, and notably in steel wire, it is very apparent.

"The wire, moreover, requires to be raised to a very high temperature before the jerk is seen on cooling. I have not observed the momentary elongation on cooling when the wire has only been heated to a point just beyond that at which it would otherwise occur.

"During the cooling of the wire it was found that just as it reached a very dull red heat, a sudden accession of temperature occurred, so that it glowed once more with a bright red heat. It was found that the reheating of the wire occurred simultaneously with the momentary elongation."

In studying these phenomena it seemed, in the first place, desirable to construct apparatus of much greater delicacy and heating-power than that used by either of the previous observers, so that, if possible, the shortening during heating could be shown with the same facility as the elongation during cooling. This has been achieved by means of the instrument a sketch of which is given with the Charts. The main idea has been to construct an apparatus which would exhibit the phenomena readily without the use of mirrors or batteries, and that would admit of experiments being performed either with or *without strain.* This latter has been accomplished by placing the wire vertically, so as to get rid of all drooping when heated. With apparatus thus constructed, we found no difficulty in displaying the phenomena in the most perfect manner, the heating-kick not unfrequently being three or four centimetres in length, and the cooling-kick sometimes as much as 13 centimetres.

It is necessary to state *in limine* that *strain* plays no part in the production of the phenomena of the kicks or jerks. Its only effect is to cause a very small influence in the cooling (which might have been overlooked with certain kinds of apparatus) to be observed. It is obvious that when a wire has a tendency to expand, the presence of weight or tension, by assisting to overcome the inertia of the apparatus, would favour the exhibition of the act; but it is equally clear that it would operate in an opposite manner when the tendency was to contract, as in the heating; and therefore, although it would favour the cooling, it would be prejudicial to the heating-kick.

Strain, therefore, being *unessential* to the phenomenon, and, by the introduction of false quantities, prejudicial to the quantitative comparison of the several effects, it has been carefully avoided in these experiments, sufficient weight alone being used to keep the thread tense upon the pulley, and never in any case to elongate the particular wire under use, even when maintained for some time at its highest temperature.

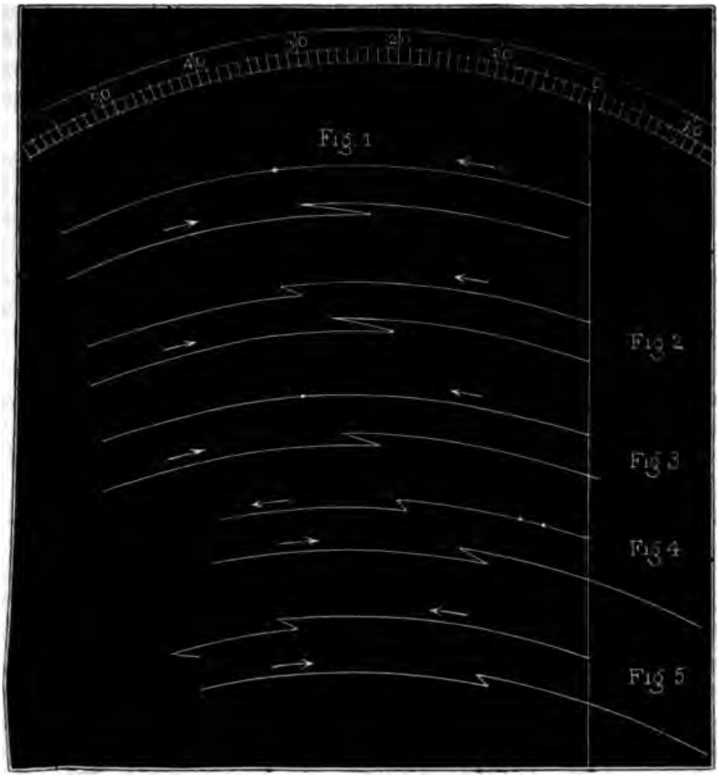
It is desirable that these facts should be borne in mind; for no sooner do we enter upon this research with more delicate and reliable apparatus, than we find that the kicks are but a small part of the phenomena we are called upon to observe and explain. We ascertain at the very outset that iron and steel wire presents itself to our examination under very different conditions, which demand a rigid analysis. Thus, apart from its inherent quality, it may come to us for experiment:—

(*a*) In its usual commercial state.

(*b*) After having been subjected to various degrees of annealing.

(*c*) In various degrees of induced hardness.

It is proposed to consider, in the first place, the phenomena presented by the ordinary steel wire of commerce, when *first* subjected to the influence of heat. During the heating of such wires, we notice that a first kick occurs at a point varying on the scale from 29° to 33°, and that the wire subsequently proceeds to a high expansion, which may be represented on the scale by the figures 66°, 52½°, 55°, 50½°, and so forth, according to the quality and state of the wire. In the act of cooling a kick commences at points varying from 31½° to 22°, and terminates at points varying from 33° to 28°, when the finger proceeds on towards zero, but *invariably falls short of reaching it by amounts varying from ½° to 5½° (centimetres).* [*Vide* fig. 1.]



In the next experiment with these wires a much altered state of things is found to obtain. Thus the first kick is lowered a little in position, increased in range; the expansion of the wire is materially diminished, being now represented by such figures as 58⅓°, 43½°, 45⅓°, 50¾°, &c. respectively. In the cooling the second kick is separated more from the first, being put back in the direction of zero. It will also be seen that in cooling the finger passes *beyond zero*, indicating now that the wire is *permanently shortened*, whereas in the primary heating it was *permanently elongated.*

A comparison of these two series of experiments will show us that, during the heating of these wires, there is going on at the same time a whole series of phenomena, some of which, although different in nature, are alike as to result, while others are different both as to nature and result. We have, in the first place, the ordinary expansion, which we will designate the "dynamical expansion." In the primary heating an amount of expansion will be seen to occur which has no equivalent in the dynamical contraction of cooling. The secondary heatings reveal to us the existence of a contraction, which is also excited *during the heating*, and the action of which is to limit the dynamical expansion previously referred to. That it *masks* a portion of the dynamical expansion is rendered obvious by the fact that, in cooling, the finger moves *beyond* zero, showing that the wire has contracted more in cooling than it expanded in heating.

In addition to this, we have also the phenomena of the kicks themselves; and these may be described as consisting of a *temporary* contraction and expansion.

During the heating of a wire, when in its commercial state, there is one expansive and two contractive tendencies exerting their powers at one and the same time; and conversely, in the *act of cooling*, there is one contractive and two expansive influences at work, viz.:—ordinary cooling contraction; an expansion which is the opposite of the temporary contraction of heating—*the cooling-kick*; and an expansion which is the opposite in nature, but not necessarily in amount, of the contraction of heating—crystalline expansion. In every case the interpretation of the general result depends upon the accurate estimate of the extent to which these interacting forces have modified each other's effects. Thus during the heating we have a temporary and a permanent contraction, assisting each other to oppose the dynamical expansion. In cooling, on the other hand, we have two expansions, one of which is temporary and the other permanent, opposing the dynamical contraction of cooling.

It will be desirable to carefully define the nature of these respective influences, and the terms which will be used to distinguish them in this research.

I. *Dynamical Expansion.*—This term will be used to distinguish the ordinary dilatation produced in bodies generally, by raising them from a given temperature to a higher one, and which is exactly counterbalanced by contraction when the original temperature is regained. It is proposed to call this kind of contraction "the dynamical contraction."

II. *Contraction of Heating.*—In the diagrams representing second heatings [*vide* fig. 2] we get the first inkling of the existence of a contractive or shortening influence excited by heat simultaneously with the expansion. In the diagrams of annealed and hardened wire [*vide* figs. 3, 4, 5] this influence becomes more obvious still. In these cases it is seen to greatly *mask* the dynamical expansion. This kind of contraction may be also displayed by protracted heating of the wire, when it exhibits itself at the end of the expansion as a slow, continuous contraction of considerable extent [*vide* fig. 5].

III. *Temporary Contraction.*—This term is applied to the cooling influence which is excited in the wire during heating, and which, at a certain point, balances the expansion; for heat being rendered latent at the same rate as it is being absorbed, produces a stop (which indicates that the expansion is arrested and balanced) or a kick (which implies an absolute cooling of the wire from excess of latency). The diminution of the rate of expansion, or the actual shortening produced by this kind of contraction, is of a temporary character, and must not be confounded with the heat-contraction which it accompanies.

These, then, are the phenomena of heating:—

(1) Dynamical expansion;
(2) Permanent or heat contraction;
(3) Temporary contraction (heating-kicks);
(4) Stretching; and
(5) The influence of oxide.

We have now to consider what influences are present during the cooling:—

I. *Dynamical Contraction.*—The opposite of dynamical expansion.

II. *Permanent Expansion* of cooling.—(Crystalline expansion.)

III. *Temporary Expansion.*—The *thermal* expansion due to the reglowing of the wire during cooling. This is the proximate cause of the stops and kicks which occur in cooling. It is evanescent as an opposing force, and must not be confounded with the permanent or crystalline expansion of cooling, of which it is simply the effect.

We have, then, in cooling:—

(1) Dynamical contraction;
(2) Permanent expansion of cooling;
(3) Temporary expansion (cooling-kicks); and to these may be added
(4) Influence of oxide.

An exhaustive study of these various conditions has led to the elucidation of the nature of hardening, softening, tempering, annealing, &c. of iron and steel, and has further shown that numerical values may be assigned to these states.

In brief, the research establishes:—

1. The existence in steel, and in iron containing *free* carbon, of a contraction or shortening which is excited by heat, and which proceeds simul-

taneously with the dynamical expansion and masks its true amount. This is divisible into *high* and *low* temperature contraction. [Compare figs. 4 & 5.]

2. The presence of a cooling expansion or crystallization, which comes in during the dynamical contraction and masks its true amount.

3. These effects, due to crystallization and decrystallization, are the causes of the so-called kicks, or temporary contractions and expansions, which occur during the heating and cooling of the steel.

4. That the low-temperature contraction and cooling expansion are due to decrystallization and crystallization which occur during the acts of heating and cooling, while the kicks themselves are simply the thermal effects associated with these changes, and are proportionate to their extent.

5. That protracted annealing (that is, *extremely slow cooling*) brings about molecular separation of the carbon and iron; and steel in such a state contracts greatly when high temperatures are reached, producing the *contraction returns* seen at the end of the heating, and which are due to the condensation produced by the recombination of the carbon and iron. Steels in this state are less susceptible to cooling-expansion (crystallization), and therefore to low-temperature contraction on subsequent heating. [*Vide* fig. 5.]

EXPLANATION OF THE WOODCUT.

Fig. 1. Commercial steel wire. First heating.
Fig. 2. Do. do. Second heating.
Fig. 3. Air-hardened wire.
Fig. 4. Water-hardened wire.
Fig. 5. Annealed steel wire. High-temperature contraction. Contraction returns.

N.B.—In each figure the upper curve refers to the heating-, the lower to the cooling-effect.

II. "On the Rapidity of Growth and Variability of some Madreporaria on an Atlantic Cable, with remarks upon the rate of accumulation of Foraminiferal Deposits." By Prof. P. Martin Duncan, F.R.S., Pres. Geol. Soc. Received March 15, 1877.

A telegraph-cable was laid off the north-west of Spain in 1870, and a portion of it was recovered in 1876, in long. 9° 4′ W. and lat. 44° 6′ N. The depth from which the recovered portion came was from 522 to 550 fathoms; the ground was conglomeratic, and there was a deposit there of sticky foraminiferal mud. Much coral growth had occurred on the cable, and when it was fished up some living and dead forms, together with *Echini*, Pectens, and mud, came up from off the surrounding sea-floor.

The growth on the cable consisted of numerous individuals of *Desmo-*

phyllum Crista-Galli of different sizes and of many bush-shaped coralla of *Lophohelia prolifera*, var. *gracilis*; there were also small masses of *Solenosmilia variabilis* (nobis), a new *Amphihelia*, and a specimen of *Caryophyllia cylindracea* (Reuss), which were not attached, but which must have been fixed close by to stones.

As the date of the sinking of the cable was known, and as six years had elapsed, it was possible to estimate the rapidity of the growth of the coral on it, and also to come to some more or less satisfactory conclusions regarding the rate of the deposit of the foraminiferal ooze in that situation. Moreover a glance at the numerous specimens showed that they presented variations and abnormalities of structure well worthy of examination, and which might relate to the inadvisability of retaining some of the specific and generic determinations in the ancient and recent coral faunas.

The height of the tallest* *Desmophyllum* taken from the top of the cable, to which its base is strongly adherent, is 1⅔ inch. It is a fine and well-grown individual, being 1½ inch in its calicular length, and its hard part weighs ⅓ oz. There are no indications of ooze having covered the base, and the granulation of the basal surface is perfect and free from any evidence of erosion.

The smallest specimen found on the cable has its calicular edge rather on one side and oblique, and it is $\frac{4}{10}$ of an inch above the attached base. It shows no trace of ooze; the other specimens, intermediate in size, usually present an excessively broad base below the peduncle, and in some it extends for nearly ½ inch on all sides. It consists of a layer of carbonate of lime, granular above and attached below to the outer coating of the cable.

Stunted bush-shaped masses of *Lophohelia* adhered by broad bases to the cable, and extended along it for many inches. The corallites composing the masses were crowded together to the height of an inch from the cable, and a few reached upwards about ¾ of an inch above the rest. Some had grown up obliquely, and others had their calices turned downwards, so that their margins were not $\frac{1}{10}$ of an inch from the cable. They must have always been above the ooze.

Gemmation appears to have occurred four times in the tallest corallites, commencing on the parent when it attained a certain size; probably the parent growth occupied one year; and there were four consecutive yearly buddings.

From these details it may be gleaned that the upward and general rate of coral growth at 550 fathoms is rapid in relation to that noticed in Europe in the same family in shallower water. In height the growth amounts to a minimum of 0·29 inch in the year, and in mass it is very considerable.

The amount of sedimentary deposit, consisting of the tests of Foraminifera, sponge-spicules, and minute particles of siliceous minerals, has been inappreciable on the cable during six years. A few Foraminifera in crevices in the bases of a few specimens are the only signs of its presence. But that there was plenty of sticky ooze close by is evident; for some was brought up by the apparatus, and it had got into parts of the calices of some of the living corals. Moreover a mass of conglomerate which was brought up, and which consisted of water-worn gneiss boulders cemented together, had some of the mud entangled in it; and most of the calices of the dead corals which were brought up at the same time, but which were not attached to the cable, contained a small quantity of foraminiferal and siliceous matter.

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* The *Desmophylla* and *Lophoheliæ* are essentially oceanic deep-sea corals; they have none of the exotheca which distinguishes the rapidly growing littoral reef-building forms

It is possible that the motion of the tentacles and of the cilia of the corals prevented the accumulation of sediment in their neighbourhood; but the tall peduncles of some of the *Desmophylla* would place their calices far out of the way of matter collecting on the base. Moreover the part of the cable on which the coral grew may have been laid on masses of stone above the level of the deposit. But the facts that the calices of the living *Amphihelia* brought up, and which was not growing on the cable, contained no deposit, and that the dead *Solenosmilia* and a short *Caryophyllia*, neighbours to the form just noticed, had very small amounts in their calices, which had long been dead, and had been worn by *Achlya penetrans* and some Spongida, are of themselves sufficient to disprove a rapid rate of accumulation. The presence of some most fragile outgrowths from the Lophohelian corals which supported and partly enclosed the stems of some Hydroida contraindicate the existence of a current sufficient to move sticky ooze.

It may be considered, then, that the deposit of minute sedimentary matter and of pelagic Foraminifera is excessively slow in its rate of accumulation at 550 fathoms on this part of the Atlantic floor, and that it is very much slower than the contemporaneous coral growth.

An examination of some of the deep-sea corals of the true *Globigerina*-ooze area will afford a corresponding observation; and we may assume that in the White and Red Chalks of England the Madreporaria grew vastly more quickly than the deposit accumulated which subsequently environed and overwhelmed them. One of Lonsdale's discoveries was that of an *Amphihelian*-looking mass from the Chalk of Gravesend*; its bulk was considerable, and yet many of the calices were close to the base, and they were those of young buds. Again, in the Red Chalk the corals are often widely open and short and were probably very slow growers. All these considerations tend to the impression that the chalk of old, whatever may have been its original nature, accumulated extremely slowly.

The variability of the specimens of *Desmophyllum Crista-Galli* which were found on the cable is very great; and in some instances it is sufficient to permit of a specific distinction being made according to the strict classificatory rules. Doubtless had the specimens been separated, and had they been assumed to have come from different localities, new species would have been made of them.

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* Suppl. to Brit. Foss. Corals, Cretaceous, Palæont. Soc. By P. M. Duncan.

Several specimens are very costulate, and there are crests to all the larger costæ; in some there are wart-like growths in those situations, and in these forms the calice is sometimes widely open, or very compressed, or normally slightly so at its orifice.

In at least one fourth of the specimens the shape of the corallum, instead of being subturbinate and compressed above the round pedicel, is tall and cylindrical; and there are no costal ridges of any importance. Moreover the size of the calices and septa varies in this series.

Some specimens, otherwise normal, have very broad basal expansions out of all proportion with the height. But the most interesting variation is noticed in those specimens which have widely open calices and exsert septa; for, added to these specific structures, are costal crests, ridges, processes, and root-like projections coming from the body, peduncle, and base. These projections are either free at their end or are attached to some support; sometimes the growths are in relation to the costal line, and in others they cannot be maintained to be so, and they are either smooth, granular, or like shagreen. There is no epitheca on the coral, and the root-like projections are therefore growths of the ectoderm. Some act as supports; but most have been produced by the irritation of an Annelid, which, after boring out of the cable came in contact with the coral, which endeavoured more or less successfully to cover it up.

Those processes which are beyond the reach of Annelids and which act as supports singularly resemble those root-like growths which are of generic or specific importance in many groups of Madreporaria.

Flabellum, *Rhizotrochus*, *Rhizophyllum*, *Omphyma*, &c. are genera which possess such root-forming species. But the root-like processes of *Flabellum* have a higher physiological interest than those of *Desmophyllum*; for some finally separate the base of the coral from its attachment by their downward growth-pressure; nevertheless the development of root-processes by the cable covering *Desmophylla* is suggestive and important, although some are morbid growths.

The cylindroid specimens would most probably be considered specifically distinct from the others were they found away from them or in strata. They are very suggestive; for in palæontology the shape of the corallum, the contour of the calice, and the relative size of the septa are often considered to establish species; and such genera as *Trochocyathus*, *Trochosmilia*, and *Montlivaltia* amongst Mesozoic corals, and *Cyathophyllia* and *Zaphrentis* amongst the Rugosa, teem with specific names which are not established on better grounds than that of the cylindroid *Desmophyllum.*

The *Lophohelia* on the cable present great bud variation; and the young and old corallites are of many different shapes, from the turbinate to the tubular. But the most important structural peculiarities are of two kinds:—first, the annelid growth has determined outgrowth of the coral which has covered in the worm-tube; and second, the establishment of some Hydrozoa on the ectoderm of the coral has sometimes produced the formation of tubes of coral-structure which environ the stalk of the offender and form a useful support to it.

Finally it may be remarked that all the Madreporaria which were brought up with the cable from off this area have an unusual ornamentation.

I have to thank Sir James Anderson for the specimens and for the details of the recovery of the cable.

III. "On Attraction and Repulsion of Bubbles by Heat." By WALTER NOEL HARTLEY, F.R.S.E., F.C.S., King's College, London. Communicated by Professor STOKES, Sec. R.S. Received February 26, 1877.

In my first paper "On the presence of Liquid Carbon Dioxide in Mineral Cavities" ('Journal of the Chemical Society,' February 1876), I mentioned having noticed a remarkable repulsion of the bubbles in fluid-cavities when they were approached by a heated body. I at first regarded these movements as similar to those observed by Mr. Sang and Dr. Hunter (Proceedings of the Royal Society of Edinburgh, 1872–73, p. 126) in cavities of Iceland spar; but with reference to the position of the source of heat, I have since found that they occurred in quite the reverse direction. The motion noticed by Mr. Sang was a repulsion of the liquid; that which I recorded was a repulsion of the gas by the heated body.

Here I may as well say that this refers to the real and not the apparent direction of the motion as seen under the microscope.

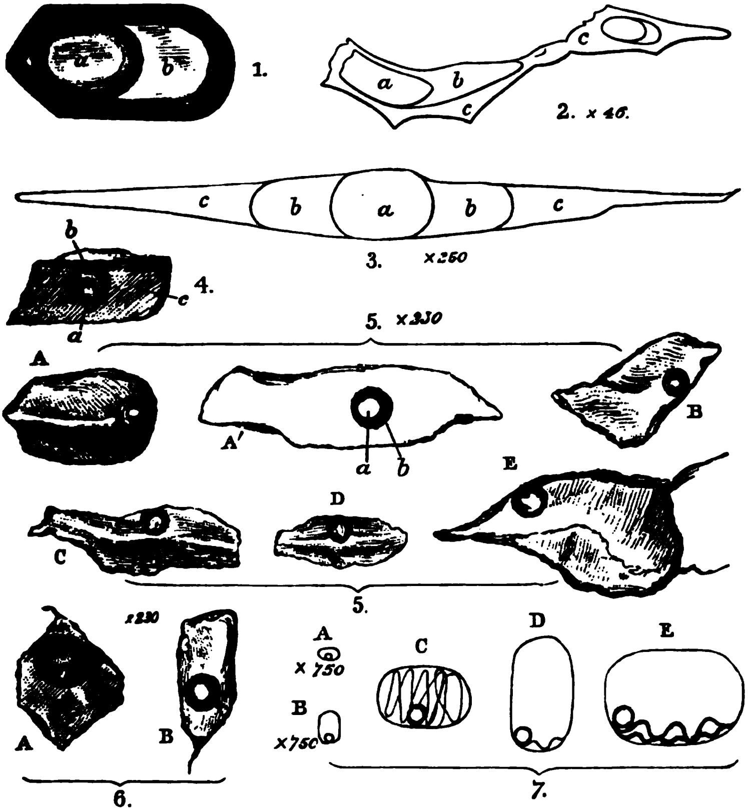
Professors Tait and Swan have shown (Proc. Roy. Soc. of Edinburgh, 1873–74, p. 247) that the attraction of the bubble by a heated body is a natural effect if the liquid be of great volatility, in contact only with its own vapour, as would be the case if the cavity were filled with carbonic acid. Distillation of the liquid would take place when one side of the bubble was heated ever so slightly above the temperature of the other, and condensation would occur on the cooler side. This would occasion a movement of the bubble from the cold to the warm side of the cavity; but it is not the original bubble being simply propelled. Professor Tait assumes that the liquid in Mr. Sang's specimens is carbonic acid, and applies this explanation. This might well be the case, because the attracting pieces of metal used were but a very few degrees warmer than the specimens acted on; but from other circumstances, some of which I propose giving in detail, I am of opinion that these were water-bubbles.

The attraction of a gas-bubble in a cavity containing liquid carbonic acid will always take place when the proportion of liquid to gas is so small that evaporation is readily effected. If the liquid at 15° C. occupies one half of the space of the cavity, this will occur only under special conditions, because the liquid in such proportions expands by increase of temperature. Thus in the case of a cavity in a topaz, shown in fig. 1, a number of experiments have invariably failed to cause any transference of the bubble from place to place. The approach of a warm substance causes immediate expansion of the liquid and decrease in size of the bubble.

The gas-bubble in a cavity of rock-crystal, shown in fig. 2, behaves quite differently; the proportion of liquid to gas is such that heat causes evaporation instead of expansion; and accordingly the liquid is repelled apparently, and the gas-bubble attracted by a heated body. With the cavity in tourmaline (fig. 3), when the heat is applied in a particular manner, the same movement takes place; ordinarily the liquid expands. To cause distillation and not expansion, the source of heat must be small and the rise of temperature slight, in order that only one end of the cavity may be heated. I have always failed to get this effect with the topaz cavity, probably because the thickness of the section causes the heat to be diffused over the liquid. The tourmaline section is thin and the cavity long and narrow, and is therefore an easy one to experiment with; so likewise is the cavity shown in fig. 5, E; it contains carbonic acid only, and the bubble is easily attracted when the source of heat is properly applied. In the course of some thousands of observations, made within the last two years, I have noticed other movements than such as may be compared with the experiments of Professors Tait and Swan on tubes of liquid sulphurous acid. The circumstances influencing these movements, and the various conditions under which they take place, render it necessary that I should disregard the order in which I observed and originally recorded them; for I have on more than one occasion been bewildered by noticing what appeared to be diametrically opposite facts in experimenting on the same specimen, and even on the contents of the same cavity. I therefore consider it expedient to classify my experiments, in order to make the account of them intelligible.

The attraction of bubbles by heat, water being the only liquid present.

With regard to the attraction of bubbles by heat, I have noticed this take place in some water-cavities when the bubbles were free to move and no carbonic acid was present. In order that no mistake might possibly occur as to the relative positions of the source of heat and the moving bubble, a hot platinum wire was used and always brought into the field of the microscope. Some thousands of cavities have been noticed occurring in sections of rock-crystal and in the quartz of various kinds of granite. The rise in temperature required to cause this movement was measured at first by blowing warm air with a ball-syringe on to the object, and then directing it on to the bulb of a delicate thermometer. With the most sensitive bubbles three or four degrees Centigrade were found to be amply sufficient.



The letters in italics (*a*, *b*, *c*) within the drawings of fluid-cavities indicate the positions of the gaseous carbonic acid, liquid carbonic acid, and water respectively.

Fig. 1. Cavity in a topaz. × 35 diameters.

Fig. 2. Cavity in rock-crystal. × 46 diameters.

Fig. 3. Cavity in tourmaline.

Fig. 4. Cavity in rock-crystal, containing a bubble repelled by heat. × 250 diameters.

Fig. 5. Six cavities in rock-crystal adjacent to each other. The bubbles in A and A′ are repelled by heat; in B, C, and D they are attracted. These cavities contain water. E contains liquid carbonic acid only. × 250 diameters.

Fig. 6. Two cavities in rock-crystal. The bubbles contain liquid carbonic acid floating on water. These bubbles are repelled or attracted by a source of heat according to the temperature of the specimen.

Fig. 7. A and B are cavities in felstone, containing liquid carbonic acid and a vibrating bubble. C, D, and E are diagrams representing the motion of the bubbles under different conditions of temperature.

The following experiments show the conditions under which such attraction takes place. In a specimen of rock-crystal from which several sections were cut, there was a multitude of cavities, many of considerable size. They all contained one liquid, water, and what appeared to be a gas-bubble, which was attracted by heat. A slice of the crystal was mounted between pieces of stout sheet platinum, and so placed on the stage of the microscope that it could be easily taken off and replaced at once in exactly the same position, so that the cavity under observation would be within the focus of the object-glass. By immersing the specimen in hot mercury, and instantly after removal examining it with the microscope, I ascertained that at 150° C. the liquid had just expanded so as to entirely fill the space. It is evident, then, that little or no gas is present.

It was next necessary to ascertain the lowest temperature of a body by which the bubble could be attracted. This was accomplished in a most satisfactory manner by selecting a cavity which was plainly visible with a 2-inch objective. A long test-tube, having a diameter of $\frac{3}{8}$ of an inch, was filled with water, immersed in which was a fine thermometer. The tube was heated, and experiments were repeatedly made while it was cooling to find out when it ceased to attract the bubble.

A number of trials showed that at 76° C. attraction was powerful, at 71° C. it was somewhat feeble, but below this temperature there was no action sufficient to overcome gravitation. Further experiments were made in a straight tube-like cavity, which from its size and its regularity of shape was exceptionally good for the purpose. It measured $\frac{34}{1000} \times \frac{7}{10000}$ of an inch, and the bubble was $\frac{6}{10000}$ of an inch in diameter. It moved about on change of position as freely as the bubble in a spirit-level. Proceeding as before, the temperature of the crystal being 16° C. and that of the tube 21° C., the bubble could be attracted in a horizontal direction only; but when the tube was warmed to 60° C. its power of attraction was sufficient to overcome the buoyancy of the bubble, and draw it downwards to the extremity of the cavity.

A piece of rock-crystal was examined which contained both water and carbonic-acid cavities in juxtaposition. These water-bubbles were very easily attracted, as will be seen by the following experiments. A cavity was chosen with a bubble moving as easily as the bubble in a spirit-level; its size was $\frac{1}{250} \times \frac{1}{400}$ of an inch, and the diameter of the bubble $\frac{1}{1500}$ of an inch.

While the section of the crystal was maintained at 16° C. a platinum wire was heated in mercury and applied to the cavity, showing the following effects:—

Temperature of wire.	Effect produced.
25°	Feeble attraction.
27	The same.
29	Strong attraction.
31	Very strong attraction.
65	Just sufficiently strong to overcome gravitation.
71	Attracted strongly in opposition to gravitation.

These numbers were confirmed by using the tube of water as a source of heat.

I have ascertained by experiment that at very slight elevation beyond the ordinary temperature a plug of water is apparently repelled from the surface of a glass tube. Mr. Sang has made similar observations.

A capillary tube, open at both ends, had a short column or plug of water within it, free to move in either direction. A warm body applied to the liquid repelled it with great force, even in opposition to gravitation. The warmth of the fingers (in other words, a rise of 21° C.) is quite sufficient to drive the liquid up a tube held in a vertical position. By sealing water in capillary tubes bubbles are formed which contain very little, if any air; these are likewise attracted by heat. When experimenting on the bubbles in natural cavities it was found that an increase of 44° to 49° C. was required to produce the same effect; but the fact must be taken into account that the heat was more difficult of application to the rock-section than to the capillary tube.

When the tube approaches $\frac{1}{8}$ of an inch internal diameter, the glass may be heated to redness at a point in close proximity to the water without causing motion; the water is, however, instantly converted into steam without previous warming, which causes a sort of slight explosion.

If this experiment be made in smaller tubes of $\frac{1}{70}$ inch internal diameter the repulsion is easily caused, and may be seen; but a very high temperature causes the repelled liquid to be evaporated and scattered in drops at a further distance along the tube.

The liquid is not repelled in a body as a liquid, but gradually as a vapour. If it had only the space of a bubble to condense itself in, the bubble would be attracted in the same way as carbonic-acid bubbles.

The attraction of bubbles in cavities which contain water may be due to two causes:—1. At low temperatures, as, for instance, at 21° C., to a repulsion of the liquid from the glass; 2. At high temperatures, such as 60° and 70° C., to evaporation and condensation on opposite sides of the bubble.

The movements of bubbles in Iceland spar noticed by Mr. Sang may thus be explained, for in that substance water-cavities are of constant occurrence. There is no necessity to assume what seems, from my observations on some hundreds of specimens of Iceland spar, to be highly improbable, namely, that the liquid is carbonic acid. The mineral is so soft and so easily split along its planes of cleavage, that I doubt whether microscope sections could contain a liquid of such high vapour-tension.

The following rocks contained bubbles in water-cavities which were attracted by heat:—Granite from the Mourne Mountains; Aberdeen granite; quartz from Snowdon; quartz-porphyry from Pwlheli, North Wales; granite from Ludgvan and St. Leven, Cornwall. Many other specimens contained immovable bubbles.

The repulsion of bubbles by heat, water being the only liquid present.

With regard to this second point, the repulsion of bubbles by heat. It occurs quite as frequently, if, indeed, not more so, in the specimens which I have examined, than attraction, and it is seen to occur in cavities containing water and liquid carbonic acid. (See fig. 4.)

I have noticed some cavities of a remarkable nature, inasmuch as they were apparent under similar conditions to those which I have already described, though they behaved in an exactly opposite manner. They were water-cavities which adjoined others containing both liquid carbonic acid and water. A blast of warm air, insufficient to vaporize the carbonic acid, is sufficient to propel the gas-bubble to the other end of the cavity. I next ascertained that five puffs of warm air only just warmed the carbonic acid to the critical point, that is to say, from 16° to 31° C. I then took a thin bar of copper, and warmed it two degrees above the temperature of the room; this repelled the bubble easily; and other trials showed that a rise of temperature of less than $\frac{1}{2}$° C. was quite sufficient. It was curious to see that when the gas-bubble touched the walls of the cavity at only one point it moved with extraordinary ease and slowly, but otherwise it was more difficult to stir, and it went with rather a sudden jerk. This subject will be treated more fully later on.

The largest specimen of a bubble readily movable by heat was in a water-cavity in a green crystal of fluor-spar kindly lent me by Mr. James Bryson, of Edinburgh. The cavity measured $\frac{1}{10} \times \frac{1}{20}$ of an inch, the bubble being $\frac{1}{27}$ of an inch in diameter. To my surprise, I found it to be easily repelled by a jet of warm air.

The sinking of gas-bubbles by rise of temperature in cavities containing water as the only liquid.

In a paper which I have lately communicated to the Chemical Society, I have given details of experiments on certain bubbles in water-cavities, which prove that by rise of temperature the bubbles become denser than the water and sink (Journal of the Chem. Soc. vol. i. 1877, p. 245).

When exposed to a uniformly diffused rise of temperature on the microscope-stage the very slow sinking motion of the bubble was remarkable; as the specimen cooled it returned in the same manner. In some cases a temperature of 40° C. was apparently sufficient; but several experiments on an exceedingly good cavity, which measures $\frac{1}{330} \times \frac{1}{410}$ of an inch, and the bubble in which is $\frac{1}{800}$ of an inch in diameter, fixed the temperature for this specimen at 150° C. The cause of this sinking appears to be that the bubble consists of a gas so highly compressed that it is nearly of the same density as water. On heating the water expands, and the gas is contracted until the relative densities of the two substances are reversed. Professor Andrews has shown that a mixture of 3 vols. of carbonic acid with 4 vols. of nitrogen at 7°·6 C. contracts $\frac{1}{378}$ of its original bulk by a pressure of 284 atmospheres. This must be a gas with a density of ·745 compared with liquid water at unity; hence, in all probability, at a tension of 400 atmospheres this gaseous mixture would be denser than water. I have elsewhere pointed out that carbonic-acid gas which was reduced to $\frac{1}{17}$ of its volume by a pressure of 223 atmospheres, at 63° C. must have been as dense, if not denser, than water (Journal of the Chem. Soc. vol. ii. 1876, p. 250).

Some of those gas-bubbles which I have already mentioned as being readily attracted by heat, I found were made to sink by warming to about 150° C. It is always necessary to rotate or at least reverse the objects when under examination; and this precaution was always strictly regarded to obviate errors of observations. The importance of this is shown by the following experiment. A bubble in a specimen of rock-crystal was seen to descend to the lower point of the cavity when it was uniformly heated from above only; it was found to be attracted by a hot spatula applied to one end of the cavity. It was thought possible that the cavity might have an oblique inclination, and be attracted from the upper end of the cavity, because this motion might bring it nearer the surface where the source of heat was placed. This was evidently the case, for on turning the slide upside down no motion was caused by uniform heating.

The following experiments were made on some good-sized cavities in rock-crystal. On presenting a heated wire to one side there was instant attraction, and then the bubbles remained at the bottom of the cavities, after which they settled slowly into their original positions. A hot spatula was passed over the specimen; the bubbles went to the bottom and there remained, in spite of the attraction of the hot spatula to the other end; they then, after cooling slightly, ascended, but descended again on removal of the spatula, as if jerked back by a spring. After a time they finally ascended slowly. This is a curious effect: it seems that the heat, if strong, causes the bubbles to sink, and that the heat of the spatula cannot attract them up until they have cooled somewhat; that after attraction has drawn them to the upper ends of the cavities, and the source of heat has been removed, they sink once more, and finally take up their original positions after further cooling.

Attraction and repulsion caused by heat in different cavities of the same specimens.

Bubbles attracted by heat and those which are repelled have generally been found in separate and entirely different specimens; and it would appear most improbable that they should exist in the same piece of stone side by side.

Fig. 5 shows six cavities, which, though not in the same field of the microscope, yet exist within a quarter of an inch square of the same section of rock-crystal. The cavity marked A′ contains water and liquid carbonic acid, and cavity E contains liquid carbonic acid only; this might be considered sufficient evidence of other cavities containing a highly compressed gas; actual experiment, however, has proved that the bubbles are spaces left by the contraction of the water on cooling from a high temperature, and therefore contain aqueous vapour and only such gas as may be dissolved in the water. Some obstruction, probably friction or adhesion of the liquid, caused by the flatness of the cavity, prevents the bubble in A′ from moving freely; but it is actually repelled, or there is a tendency to repel it, if a wire very strongly heated be brought near. It is not attracted, however, at any temperature. When repelled it returns as if squeezed back. Capillarity makes the bubble assume a spherular form whenever possible; therefore it returns to such a position as is most compatible with this shape. Sometimes the motion is not a transference of the bubble from one point to another; it seems to be fixed, but flattened at one side, and shaken as if something were pushing and trying to move it. The bubble in A, a deeper cavity, moves very freely and is repelled by heat. The cavities B, C, D contain bubbles which, curiously enough, are attracted by heat.

Another cavity of irregular shape, and at least four times the size of the largest of these, behaved exactly in the same manner. As in the other experiments, the objects were frequently turned about in different directions to prevent mistakes.

A series of experiments were made on these cavities to ascertain the precise difference, if any, between them.

The bubble in A was found to have disappeared at 105° C., and it returned immediately on cooling with a sort of jump, which carried it the whole length of the cavity, and made it rebound from the further end. At 104° C. the bubble had not disappeared. These numbers are the result of sixteen experiments.

The bubbles in the cavities B, C, D did not all behave in the same way. Thus, from ten experiments at different temperatures, it was found that at 85° C. the liquid in C had expanded so as to fill the entire space, at 83° C. it had not done so, while D required a temperature of 123° C. The liquid in B was apparently unaffected by so slight a rise of temperature, but it was made to fill the cavity at 138° C. When heat had been applied so that the bubbles in all the cavities had disappeared, the one in B returned first, that in C generally appeared next, and that in D last.

Sometimes, after very strongly heating the specimen, the bubbles in C and D did not return for half an hour, though two or three minutes was a period quite sufficient for the specimen to become cooled down.

Sometimes the appearance in the cavity on cooling somewhat resembled the sort of ebullition which occurs when carbonic acid is cooled when at a temperature above its critical point; the motion, however, of the bubbles was much slower, and occurred in one direction only, except when the bubbles rebounded from the lowest point of the cavities.

The bubble in B does not roll about when the microscope-stage is rotated; in this respect it differs from those in C and D. Careful experiments were made with the view of ascertaining the temperature producing repulsion and attraction respectively in the different cavities of this specimen.

Repulsion to the extreme end of the cavity, entirely in opposition to the effect of gravitation, was produced by a temperature of 5° C. above that of the specimen. Attraction in opposition to gravitation in cavity D became active by a rise of 5° C.; in B 14° C. were insufficient to do more than give a lateral motion to the bubble. On cavity C 12° C. acted energetically.

A series of experiments were made on bubbles which contained liquid carbonic acid as well as gas.

By heating the specimen above the critical point of the carbonic acid we know something of the conditions under which subsequent experiments may be made. We know that the liquid is water containing a gas-bubble under a pressure of not less than 109 atmospheres. The following are facts which, like those preceding, were recorded at the moment of observation. Fig. 4 represents a cavity in rock-crystal with carbonic acid in the liquid and gaseous states floating upon water. The bubble is so easily movable that it shifts about like the bubble in a spirit-level. The stage of the microscope holds the section in a vertical position, and when one end of the cavity is raised ½ a degree Centigrade in temperature, the bubble is driven to the opposite extremity; if the specimen be turned over, this will happen in spite of the buoyancy of the bubble. The bubble takes up its original position on cooling. When the specimen is uniformly heated above the critical point of carbonic acid, repulsion by heat still takes place. I have repeated this experiment during the last twelve months an immense number of times, both with fine jets of warm air and with platinum wires, always with the same result. Another exactly similar cavity being under examination, heat was applied by means of a hot wire spatula. When the edge of the spatula was seen to approach, there was an instant repulsion of the bubble from the upper to the lower end, and the liquid carbonic acid was vaporized. After removal of the source of heat, the bubble did not rise (*i. e.* apparently sink) to its original position until after the liquid had condensed again; it then slowly moved back. This experiment was repeated again and again with other bubbles in the same specimen, and notes were made each time to secure a truthful record. The action in every case was precisely the same; repulsion occurred, and the bubbles sank under a uniformly diffused rise of temperature. In another specimen of rock-crystal were seen two cavities, one containing water only, and the other water with carbonic acid; the bubble in the latter cavity was repelled by heat (no experiment was made to ascertain whether it sank on warming), but that in the water-cavity was attracted. In order that there might be no possible mistake about this, the two cavities were brought into the field of view at the same time, and the heated spatula approached them both from the same side; they then instantly darted in opposite directions. The movements were unaffected by raising the temperature above the critical point of carbonic acid. It is certainly very perplexing to find two cavities in the same section closely adjacent to each other, and nearly of the same size, the bubbles in which are moved in opposite directions by the same source of heat applied from the same side.

Bubbles containing gas at high tension, under different conditions of temperature, are first repelled and then attracted by a heated body.

My work was discontinued for a period of some months; but on being able to look over my specimens once more, I verified all my former observations, and became surprised by the following discovery. A bubble which was repelled by a gentle heat was attracted after it had been heated more strongly, and then on cooling it was again repelled. It appeared to contain some liquid carbonic acid floating on water with the gas. Searching for such other specimens, the cavity, fig. 6, A, was met with; it contained a large proportion of liquid carbonic acid, with a little in the gaseous state floating on water, and the bubble is so movable as to act like a spirit-level. On cautiously applying a warm spatula the bubble was repelled; on heating it a little more, the liquid carbonic acid became gas, and the bubble was again repelled. The spatula was then made almost red-hot and applied; the bubble was then strongly attracted; after cooling somewhat it was again repelled. It was noticed that after the critical point of the carbonic acid had been reached, the bubble sank through the water. It has been shown by the various experiments already related that at only moderate temperatures both repulsion and attraction can occur.

Temperature, then, does not directly cause these opposite effects; it can only be some alteration in the conditions of experiment caused by rise of temperature. Increase of tension or pressure within the cavity is apparently the only condition which has varied; and probability that this is the cause of this contradictory attraction is afforded by the following experiment. The specimen was placed in a water-oven (the temperature it would there acquire would be about 94° C. to 96° C.); the platinum spatula was heated in a beaker of oil to 130° C. The warm specimen insulated by india-rubber was placed on the microscope-stage, and the warm spatula presented to it, when instant attraction was seen. The same proceeding was repeated many times, always with the same result. As the spatula cooled it ceased to affect the bubble at all; of course at the same time the specimen was also cooling. The spatula at the temperature of 130° C. was applied to the cooled specimen, which was, however, still at a temperature above 30° C.; the effect was repulsion as at first.

This seems to show that the temperature which the rock-crystal attained in the water-oven, and not that communicated to it by the spatula, caused attraction.

To ascertain at what temperature attraction became repulsion, and *vice versâ*, the specimen was placed upon a Stricker's hot stage, and the platinum wire was heated in oil contained in a test-tube.

A succession of experiments yielded the following notes:—

Temperature of crystal.	Temperature of wire.	Effect on bubble.
40° C.	110° C.	Repulsion feeble.
42	110	„ „
45	110	Neither repulsion nor attraction.
50	100	„ „ „
52	100	Feeble attraction.
52	100	„ „

On another occasion the wire was maintained at 100° C., and a number of experiments gave like results:—

Temperature of crystal.	Effect on bubble.
42° C.	Feeble repulsion.
45	Neither attraction nor repulsion.
50	„ „ „
53	Slight attraction.

Above and below these limits attraction and repulsion were feeble.

I next ascertained the critical point of the carbonic acid in this cavity and found it to be as low as 21° C. Prof. Andrews kindly informs me that 14 per cent. of nitrogen lowers the critical point of carbonic acid to about 20° C. It is by no means unlikely that nitrogen is the gas present in this cavity in something like the same proportion, and that the tension is something very considerable.

It seems to be a matter of great interest to know whether the difference in temperature between the attracting wire and the bubble was so slight as when repulsion occurred; the specimen was therefore heated on the Stricker's stage to such a temperature as to ensure attraction by a hotter body. The following is an account of the experiments. The bulb of a fine thermometer was often used as the attracting body; at other times a platinum wire heated in mercury.

Temperatures		
of crystal.	of attracting body.	Effect on bubble.
45° C.	73° C.	Repulsion.
50	83	Attraction strong.
50	78	Bubble attracted horizontally.
57	74	Attraction against gravitation.
55	65	No movement.
55	75	Strong attraction.
60	65	No movement.
65	75	Attraction in any direction.
65	73	" " "
65	70	Attraction.
65	69	No movement.
67	73	Attraction.
67	76	Attraction strong.
68	75	Attraction against gravitation.
65	72	Attraction.
70	75	No movement.
73	78	Attraction.
60	65	Attraction feeble.

Other experiments of the same kind were made on other cavities, which were, however, more difficult to operate on, being smaller in size and of less regular shape.

Temperatures		
of crystal.	of attracting body.	Effect on bubble.
60° C.	130° C.	Neither attraction nor repulsion.
100	180	Very strong attraction against gravitation.
100	160	
100	160	
100	140	Somewhat feeble attraction.
100	140	
100	130	No effect.

Another cavity :—

75	140	Very strong attraction.
75	130	Attraction feeble.
60	130	Attraction feeble.
60	125	
60	125	

Here let me explain that attraction or repulsion, when expressed as being feeble, should really be understood as causing a slow motion. It was noticed that bubbles without gaseous contents, which were attracted, moved with a uniform motion, and were kept at the further ends of the cavities until an equilibrium in temperature had been established, so that sometimes the liquid had the deceptive appearance of sinking under the rise of temperature. When gas-bubbles were repelled by heat, their speed appeared to be accelerated after they once commenced to move.

It may be considered an argument against the motions being due to any pyroelectric conditions of the minerals, that they have been noticed in crystals of fluor-spar, and that, no matter in which direction sections of rock-crystal are cut, the movements are all equally well obtained.

Regarding the repulsion of gas-bubbles two facts are striking, namely the very slight rise of temperature (less than $\frac{1}{2}$° C.) on one side of the bubble capable of causing the movement, and the great tension existing within the bubble.

Note.—Received April 13, 1877.

I am much indebted to Prof. Stokes for having furnished an elucidation of the cause of these movements, which is perfectly consistent with all the facts which I have noticed. In consequence of this I have discarded my own explanations, which were originally embodied in the foregoing paper, but which were never perfectly satisfactory to me.

In notes dated April 7th and 12th Prof. Stokes says:—"It seems to me, as far as I can judge without having seen the specimens, that the greater part, if not the whole, of the motions you describe are referable to a cause different from that suggested by Prof. Tait, and that they depend on capillarity. We know that the surface-tension of a liquid is diminished as the temperature is raised. The explanation, according to this view, would be very similar to Professor James Thomson's beautiful explanation of the tears of wine (Reports of the British Association, 1855, Report 2, p. 16), only here difference of temperature takes the place of difference of strength, and the surface of the liquid surrounding a bubble shrinks at the cooler side." The shrinkage of the liquid on the cooler side of the bubble of course propels it towards the source of heat. This explanation seems quite in accordance with all phenomena of attraction of bubbles, whether in carbonic acid or in water, with the movement of water in capillary tubes, and the vibratory movements of minute bubbles described in the next paper.

"In the case of a cavity containing water with a bubble of compressed carbonic acid, the water, of course, containing gas in solution, the repulsion by heat may be accounted for by a slight evaporation of the dissolved gas at the surface weakening the solution, and thereby increasing the surface-tension; and it is quite conceivable that at different temperatures one or other of these opposite effects may prevail."

Conversely, one can understand how different conditions of gaseous tension in bubbles, and the extent to which the surrounding water is charged with gas, may render the effect of heat either repulsion or attraction.—W. N. H.

IV. "On the Constant Vibration of Minute Bubbles." By WALTER NOEL HARTLEY, F.R.S.E., King's College, London. Communicated by Prof. STOKES, Sec. R.S. Received March 3, 1877.

Those who have given great attention to the study of fluid-cavities in minerals have occasionally met with vibrating particles which are apparently bubbles.

I first became acquainted with these at the close of last year, 1875, when Mr. P. J. Butler was kind enough to show me a ruby containing a cavity partially filled with liquid carbonic acid, the bubble in which, when of small size, was in constant motion. I was much struck with this, and immediately connected it with the motions caused by heat which have just been described, but which at that time had not been so fully studied.

During this last summer I have had the opportunity of examining a specimen of felstone from Snowdon, for the loan of which and for a section of the same stone I am much indebted to Mr. J. C. Young, F.G.S. This contained portions of quartz with many cavities. The majority of these were water-cavities, but others appeared to be empty; and in one of them Mr. Young had noticed a moving particle, supposed to be a bubble, which made its appearance only in a cold atmosphere. By dropping a little ether on the object, the evaporation cooled it sufficiently to condense a liquid in the cavity, and the moving particle was easily seen with a magnifying-power of 400 diameters. By immersion in iced water, the temperature of which was 3°·5 C., the cavity has the appearance of being two thirds filled with a liquid, the gas-bubble of course occupying the remaining space and having a sort of trembling motion. The bubble decreased in size, and the motion became more and more rapid as the size became smaller, until it rushed up and down and across the space in which it was confined. The cavity and the bubble are represented in fig. 7 (p. 139) by A, and the course of the bubble during rapid motion by C. The thought immediately occurred that this was not a gas-bubble, but a liquid in the spheroidal condition—in all probability carbon dioxide in a perfectly dry condition, and perhaps mixed with some incondensable gas, so that its critical point was lowered. A considerable number of experiments was necessary in order to arrive at a judgment as to whether the moving particle was liquid or gaseous; for if it were the former the particle would diminish by rise of temperature through evaporation, if the latter through contraction of the bubble consequent on expansion of the surrounding liquid. Variations in temperature caused the following changes:—

The particle was moving rapidly at	13°·5 C.
Well seen at	15·5 C.
Moving particle very small, motion very rapid at	17·5 C.
Particle not visible	20·5 C. 19·5 C. 18·7 C.
Just visible	17·7 C.
Invisible	18·0 C.

At −14° C. the moving particle appeared to occupy nearly half the cavity; its motion was very slight; had it not been previously seen in rapid motion, it would have been impossible to say with certainty that it moved at all. At −15° C. it was as nearly as possible motionless. It was now necessary to watch the effect of gravitation upon the particle; for if it be gaseous it will have a tendency towards the upper part of the cavity when the liquid condenses, while if it be liquid it will more frequently be observed towards the lower end. In eight out of ten experiments in which the liquid was condensed from the gaseous state, the particle took up a position at the top of the cavity. The microscope was placed at an angle of 45°, and the stage was rotated partially before each experiment in order to avoid examining the specimen in one position only. As, therefore, the spherule is not the fluid with greatest density, it is most probably a gas-bubble, and the contents of the cavity are in such a state of compression that the liquid expands so as to entirely fill the cavity at 18° C. The liquid is doubtless carbon dioxide. Several other cavities of the same kind were noted.

This shows that in examining rock-sections it is necessary to cool as well as warm the specimens.

The following test gave indisputable evidence that the moving particle was the bubble. While in rapid motion the bubble was strongly attracted to the side of the cavity when a body of a slightly higher temperature was made to approach it. That the liquid which exhibits such motion need not be a condensed gas is shown by the following facts. A piece of quartz from the summit of Snowdon abounded in water-cavities of different sizes. Some of these contained a bubble in constant vibratory motion at 18° C. The size of the cavities was about $\frac{1}{8000} \times \frac{1}{10000}$ of an inch, and the size of the bubbles $\frac{1}{25000}$ of an inch in diameter (see fig. 7, A and B). The movements were chiefly vibrations up and down, and gentle alterations in position from right to left. A platinum spatula was made red-hot and brought near to the object-glass; the motion instantly ceased, and the bubble was seen clinging to the side nearest the heated spatula. On changing the position of the spatula the bubble did not immediately move; it seemed to require some time for a rise of temperature to overbalance that which the stone had previously acquired. From right to left and from top to bottom the bubble was attracted and made to come to rest.

When the heating was not excessive the bubbles did not become motionless, but their vibrations had a tendency to one side of the cavity, as in fig. 7, D and E.

The larger bubbles in other cavities were likewise attracted. The vibrating bubbles were generally seen at or near the top of the cavities. Cooled with ether-spray the bubbles ceased vibrating. The microscope-stage was inclined at an angle of 45° throughout the experiments. About fifty different cavities, none of them exceeding the dimensions already given, all behaved in the same way. There were several moving bubbles in cavities a little larger than those already noted; for instance one measured $\frac{1}{4000} \times \frac{1}{5000}$ of an inch. The motion never extended across the cavity, but was confined to a sort of shuffling up and down, which shifted the bubble from one side to the other (see fig. 7, D and E). On presenting a warm wire to the cavity the bubble was instantly attracted, and it remained clinging for some time to the side of the cavity.

For an explanation of the cause of vibration I must refer to the fact that I have proved, that gas-bubbles in water as well as in carbonic acid may be attracted by a source of heat giving an extremely slight rise of temperature. It is impossible to imagine a body which is not gaining or losing, or, at the same time, both gaining and losing heat; it is therefore impossible to imagine it entirely throughout at a uniform temperature. It is evident, then, that an easily movable particle which can be set in motion by exceedingly slight rises of temperature will make the transference of heat from one point to another plainly visible. I have shown that the minute bubbles in fluid-cavities are such particles; and I believe that the vibratory motions which I have described afford an ocular demonstration of the continual passage of heat through solid substances.

April 19, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Putrescent Organic Matter in Potable Water." By GUSTAV BISCHOF. Communicated by E. FRANKLAND, F.R.S. Received March 17, 1877.

It is fortunate that smell and taste are generally extremely sensitive indicators of putrefaction in articles of food. This does not, however, apply to drinking-water, which may be largely polluted by putrescent organic impurities without causing any suspicion to our senses. And yet the question of the wholesomeness of water hinges mainly upon the presence or absence of such putrescent matters, as they themselves are the cause of derangements of the human system. Most serious, however, are the consequences when those low forms of organic life, which in all probability form the specific poison of cholera, typhoid fever, and other diseases, gain admission to drinking-water polluted by putrescent matter.

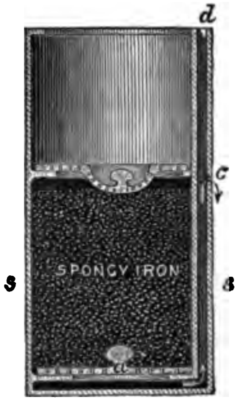
A number of observations point to the conclusion that these organisms, or their germs, are not infectious as long as surrounded by *fresh* organic matter; but as soon as fermentation sets in, they show their poisonous virulence. Thus it has been observed that the discharges of cholera and typhoid patients are not infectious as long as they are fresh, but by putrescence their poisonous character is developed.

Chemical analysis is incapable of discriminating between living or dead, fresh or putrescent organic matters. The microscope reveals their nature more fully; but it is nevertheless frequently a matter of great difficulty to decide as to the existence or non-existence of *Bacteria* of putrefaction, or their germs, in water. It thus appeared to me that this information might, in some cases at least, be gained with greater certainty by an indirect method.

If we want to determine whether a gas be carbonic anhydride, we pass it through potash bulbs, and see whether these increase in weight. Similarly the presence or absence of putrefactive agencies in water may be determined by their action upon organic matter. The test I selected is fresh meat, as the slightest putrescent changes in it can most readily be detected by its smell.

The experiments, which were originally made with a view of determining the improvement of water by certain filtering media, were, with the exception of experiment VIII., carried out in the following manner:—

On to the perforated bottom (*a*) of a stoneware vessel (*s s*) I place



some fresh meat. The vessel is then filled to about two thirds with the materials to be experimented upon, and lastly with water. Into opening *c* a tin tube is fixed, which is first bent upwards and then downwards in the shape of an inverted U, to prevent any *Bacteria* or their germs from passing through this outlet-tube into the bottom of the vessel. The air-pipe *d*, down to *c*, is filled with firmly compressed cotton-wool, and a glass tube, sealed at its bottom, is passed down through the material experimented upon, to allow of the temperature being measured in close proximity to the meat. The vessels thus prepared are immersed in a boiler filled with cold water, which is gradually heated and kept boiling for several hours. The object of this is to destroy any germs adhering to the meat. The temperature at the bottom of the sealed glass tube was, during the boiling, in each of the following experiments 93°–95° C.

After cooling, the Chelsea Company's water was constantly passed through the vessels in the direction indicated by arrows, at as nearly as possible a uniform speed.

It is thus evident that any *Bacteria* of putrefaction, or their germs, in the water would, after a time, render the meat putrid, or, if it remains fresh, they must have been absent, or at least inactive, when the water reached the meat.

I now proceed to describe the experiments.

Experiment I.—One of the vessels was filled with spongy (metallic) iron, and treated as before described; after a fortnight the meat was perfectly fresh.

Experiment II.—A vessel filled with animal charcoal; after a fortnight the meat showed strong evidence of incipient putrefaction. As experiments I. and II. were conducted side by side, this result proves that the preservation of the meat in experiment I. was not due to any external cause, such as the low temperature then prevailing.

Experiment III.—Water continuously passed through a vessel filled with spongy iron for four weeks; even then the meat was perfectly fresh and hard.

Experiment IV. was a repetition of II., the filtration of water through animal charcoal being continued for four weeks. The meat was soft and quite putrid. In the course of this experiment the exit-tube was several times choked by mucous matter.

Experiment V.—In experiments I. and III. with spongy iron, this material was employed without separating any of the fine dust. In order to ascertain whether *Bacteria* were merely mechanically retained, a vessel was charged with spongy iron, from which all the finer particles had been separated by a sieve with thirty holes on the linear inch. The filtering medium in this case was therefore of a porous nature. After four weeks' filtration the meat was perfectly fresh.

Experiment VI.—In the previous experiments with spongy iron the meat was in contact with water, from which the iron in solution had not been separated. With a view of ascertaining whether the iron in solution was the preserving agent, a stoneware vessel was charged underneath the spongy iron with pyrolusite and sand, so as to abstract the iron from the water before it came in contact with the meat. After four weeks' filtration the latter was found perfectly fresh.

Experiment VII.—By a separate experiment I ascertained that the oxygen is completely abstracted from water during its passage through spongy iron. In order to determine whether the absence of oxygen be the cause of the preservation of the meat, and whether the *Bacteria* or their germs be killed or can be revived when supplied with oxygen, an evaporating-basin was inverted over the meat. This must have retained a quantity of air in its cavity, the air being gradually dissolved by the water in close proximity to the meat. After four weeks' filtration the meat was perfectly fresh; I succeeded in collecting a small bubble of the gas, still in the cavity of the evaporating-basin. This was quite free from oxygen.

It is therefore doubtful whether oxygen was supplied to the water sufficiently long to justify any conclusions from this experiment. However, the result of experiment VIII. rendered a repetition unnecessary.

Experiment VIII.—Fresh meat was placed at the bottom of a glass vessel and left standing, covered with about four inches of spongy iron and water. The vessel in this instance was *not* boiled. After three weeks the meat was very bad, demonstrating that the action of the *Bacteria* of putrefaction adhering to the meat was not prevented by the spongy iron above; and if, during the previous experiments with spongy iron, agencies capable of causing putrefaction had at any time come in contact with the meat (in other words, if the *Bacteria* had not been killed in their passage through spongy iron), the meat must, as in this last experiment, have shown marks of their action. It therefore appears that *Bacteria* are permanently rendered harmless when passing in water through spongy iron. This conclusion is further corroborated by the observation that even effluent sewage-water, after passing through the spongy material, has remained perfectly bright for now five years when exposed to light in a half-filled stoppered bottle.

I believe that the action of spongy iron on organic matter largely consists in a reduction of ferric hydrate by organic impurities in water. We know that even such organic matter as straw or branches is capable of reducing ferric to ferrous hydrate. We know that even such indestructible organic matter as linen and cotton fibres is gradually destroyed by rust-stains. This action is slow when experimenting upon ordinary ferric hydrate; but it may, *in statu nascendi*, be very energetic—the more so, if we consider the nature of the organic matter in water. Ferric hydrate is always formed in the upper part of a layer of spongy iron when water is passed through that material. The ferrous hydrate resulting from the reduction by organic matter may be re-oxidized by oxygen dissolved in the water, and thus the two reactions repeat themselves. This would explain why the action of spongy iron continues so long.

It is, however, quite certain that there is also a reducing action taking place when ordinary water is passed through spongy iron. This is clearly indicated by the reduction of nitrates.

Our knowledge of those low organisms which are believed to be the cause of certain epidemics is as yet too limited to allow of direct experiments upon them. It is not improbable that, like the *Bacteria* of putrefaction, they are rendered harmless when water containing them passes through spongy iron; but until we possess the means of isolating these organisms, this question can only be definitively settled by practical experience. Should this not be satisfactory, should those specific contagia not be destroyed when passing in water through spongy iron, then the separation of *Bacteria* by spongy iron may afford means of isolating those germs of disease; should it be favourable, then we shall have found in spongy iron the material to prevent the spreading of epidemics by potable water.

II. "On a Cause for the Appearance of Bright Lines in the Spectra of Irresolvable Star Clusters."

By E. J. STONE, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

Received March 20, 1877.

Before the announcement of Mr. Huggins's discovery of the presence of bright lines in the spectra of nebulæ, it was generally, if not universally, accepted as a fact that nebulæ were merely stellar clusters irresolvable on account of their great distances from us. This view had become impressed on the minds of many of our greatest observing astronomers in the progress of their work, and is one therefore which should not lightly be abandoned.

It appears to me that Mr. Huggins's observations, instead of being inconsistent with the view formerly held by astronomers, are rather confirmatory of the correctness of that view.

The sun is known to be surrounded by a gaseous envelope of very considerable extent. Similar envelopes must surround the stars generally. Conceive a close stellar cluster. Each star, if isolated, would be surrounded by its own gaseous envelope. These gaseous envelopes might, in the case of a cluster, form over the whole, or a part of the cluster, a continuous mass of gas. So long as such a cluster was within a certain distance from us, the light from the stellar masses would predominate over that of the gaseous envelopes. The spectrum would therefore be an ordinary stellar spectrum. Suppose such a cluster to be removed further and further from us. The light from each star would be diminished in the proportion of the inverse square of the distance; but such would not be the case with the light from the enveloping surface formed by the gaseous envelopes. The light from this envelope received on a slit in the focus of an object-glass would be sensibly constant, because the contributing area would be increased in the same proportion that the light received from each part is diminished. The result would be that at some definite distance, and all greater distances, the preponderating light received from such a cluster would be derived from the gaseous envelopes and not from the isolated stellar masses. The spectrum of the cluster would therefore become a linear one, like that from the gaseous surroundings of our own sun. The linear spectrum might, of course, under certain circumstances, be seen mixed up with a feeble continuous spectrum from the light of the stars themselves.

It should be noticed that, in this view of the subject, the linear spectrum can only appear when the resolvability of the cluster is at least injuriously affected by the light of the gaseous envelopes becoming sensibly proportional to that from the stellar masses, and that in the great majority of such cases it would only be in the light from the irresolvable portions of the cluster that bright lines could be seen in the spectrum.

The changes in form which would be presented to us by such a nebula might be expected to be small. These changes would depend chiefly upon changes in the distribution of the stellar masses constituting the cluster. It has always appeared to me difficult to realize the conditions under which isolated irregular masses of gas, presenting to us sharp angular points, could exist uncontrolled by any central gravitational mass without showing larger changes in form than appear to have been the case with many of the nebulæ. In my view of the nature of nebulæ this difficulty no longer exists.

Royal Observatory, Cape of Good Hope,
February 9, 1877.

III. "On some Figures exhibiting the Motion of Vibrating Bodies, and on a New Method for determining the Speed of Machines." By HERBERT M'LEOD, F.C.S., Professor of Experimental Science, and GEORGE SYDENHAM CLARKE, Lieut.R.E., Instructor in Geometrical Drawing in the Royal Indian Engineering College, Cooper's Hill. Communicated by Prof. DUNCAN, F.R.S., Pres.G.S. Received April 5, 1877.

If the image of a point of light or of a black dot on a white ground be observed in a vibrating mirror, the motion of which may be produced by a tuning-fork or reed, the point, in virtue of the retention of the image on the retina, will appear as a straight line. If, however, the luminous point be moving in a direction at right angles to the plane in which the fork vibrates, and parallel to the plane of the mirror, the combination of the two rectilinear motions will produce a sinuous line or wave-form.

The dimensions of this wave will depend on the amplitude of the vibrations of the fork and on the velocity of movement of the point of light in relation to the period of the fork: when the rate of translation of the point is great the wave-length will be great; with a low velocity the wave-length will be small. The greater the amplitude of the vibrations of the fork, the greater will be the amplitude of the wave.

When a series of equidistant points attached to a rotating disk or cylinder is employed, the properties of the wave differ very much according to the velocity of the moving disk. If the points are placed in a circle on a disk or cylinder rotating with such a velocity that the time occupied by a point in passing over a distance equal to that between two consecutive points is exactly equal to the period of one complete vibration of the fork, a continuous stationary figure is perceived; but if the point passes over a distance slightly greater than the intervals, the figure will show a slow progression in the direction of the moving circle; and when the space described by the point is slightly less than the distance between the points, the motion of the figure will be in the opposite direction. If the velocity of the circle is one half of that necessary to produce the stationary wave another stationary figure results, but with half the wave-length of the previous one, the luminous point passing over a distance equal to the interval between two points during two vibrations of the fork; the brightness of the wave is also less. Generally, if the time occupied by the point in moving through a distance equal to that between two consecutive points is an exact multiple of the period of the fork, stationary waves will be formed; waves thus produced have been observed with velocities $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, and $\frac{1}{6}$ of that necessary to generate the wave-form first described.

When the velocity of the moving points is so much increased that the eye is unable to follow the movement of the figure, a complex form is produced by the overlapping of several waves; and at some velocities these figures may appear stationary, but their complexity usually makes it difficult to establish their exact form. Another simple and easily recognized figure, however, is observed when the velocity of the moving points is once and a half as great as that necessary to produce the stationary single wave. This figure is caused by the overlapping of three waves so placed that the crest of one is over the crossing of the other two. This figure, like the previous one, shows a direct or inverse motion when the velocity of the points is not exactly that which would produce the stationary figure. When the velocity of the points is such that the distance traversed by one point during a complete vibration of the fork is equal to double the interval between two consecutive points, a figure is formed by the overlapping of two waves with their crests and hollows opposed and crossing at the line that would be described by the points if the mirror were not vibrating. When this figure is produced the number of points passing in a given time is twice the number of vibrations of the mirror; this is the most useful form for observation, as it is very easily recognized. By still further increasing the rapidity of passage of the points a triple figure is again produced when the rate of rotation is exactly double that which forms the similar figure mentioned previously; or, calling the velocity producing the single figure 1, the triple is observed with a velocity of $1\frac{1}{2}$ and also 3. When the rate is 4, a figure composed of four overlapping waves is formed.

It is thus seen that, like the single wave, the triple figure may be produced by more than one velocity—in fact it has been observed with four distinct velocities, namely, 3, $\frac{3}{2}$, $\frac{3}{4}$, and $\frac{3}{5}$, taking the velocity producing the single figure as 1. Similarly the double figure has been obtained with a velocity of 2, $\frac{2}{3}$, and $\frac{2}{5}$; and the quadruple with the velocities 4 and $\frac{4}{3}$. It is obvious that these are not the only rates capable of forming these figures. Theoretically each may be obtained by an infinite number of velocities; and the relation between the number of vibrations and the passage of the points may be expressed in the following terms:—

A single wave is formed when a whole number of vibrations takes place in the time of the passing of a point over one interval.

A double figure is formed when a whole number of vibrations takes place in the time of the passing of a point over two intervals, provided that this whole number is not divisible by two.

A triple figure is formed when a whole number of vibrations takes place in the time of passing of a point over three intervals, provided that the whole number is not divisible by three.

Lastly, a quadruple figure is formed when a whole number of vibrations takes place in the time of passing of a point over four intervals, provided that the whole number is not divisible by two or by four.

Besides the mirror other means may be employed to view these figures: for instance, they may be observed through a lens attached to one of the prongs of the fork, or a real image of the figures may be produced by the lens and observed with or without a fixed lens. Instead of a tuning-fork a reed may be used to carry the mirror or lens.

From the foregoing it will be seen that the formation of these figures may be employed for determining the speed of revolution of the disk or cylinder, if the period of the fork or reed, and also the number of points on the rotating body, are known.

For some velocities, indicated by whole numbers per minute, the circle on the disk or cylinder must be divided into equal intervals; thus in the case of a fork vibrating 60 times a second, or 3600 times a minute, it is necessary that 7200 points should pass in a minute in order to form the double figure. If the disk is rotating 100 times a minute, it is clear that the circle must contain 72 equal intervals; for a velocity of 96 revolutions per minute 75 equal intervals; but for the intermediate numbers of rotations fractional numbers of intervals are necessary; for 97 revolutions 74·226 intervals are required. Great difficulties would be found in dividing the circles in this manner; and the employment of whole numbers of intervals would be equally inadmissible, 74 intervals, for example, corresponding to 97·2973 revolutions. This difficulty may be obviated by ruling on paper convergent lines, and then wrapping the paper round a cylinder, so that one of the lines is parallel to the axis. In this way circles traced round the cylinder are divided into any required parts; and between those circles, where numbers of equal intervals are found, there will be every conceivable division between these numbers.

This method of division possesses also the great advantage that equal distances along the cylinder correspond to equal differences of numbers of rotations: taking the above example, the circles on the cylinder where 72 and 75 equal intervals are found will correspond to 100 and 96 rotations respectively; on dividing the distance between the two rings into four equal parts, numbers of spaces corresponding to the whole numbers of rotations, 99, 98, and 97, will be found on the three intermediate circles. The lines may now be converted into dots by erasing the portions not required, or a screen with narrow slits placed in front of the cylinder will effect the same object.

Another arrangement may be employed by which fractions of rotations may be measured. If the lines are viewed through a narrow slit in a piece of black paper or thin metal attached to a tuning-fork or reed, vibrating in a plane parallel to the axis of the cylinder, the figures will be perceived on looking through the slit. If the fork be now moved parallel to the axis of the cylinder until the figure appears stationary, the numbers of rotations may be read off from a graduated scale.

When the figure is formed on circles, the circumferences of which are not an exact multiple of the intervals, so that one division is smaller than the rest, the figure is observed to make a sudden movement or jump at the time of the passage of the small division. There seems to be no way of preventing this; but it is not found to have any practical objection; for when the crossings of the double curve remain stationary during the remainder of the revolution, the proper position on the cylinder has been obtained. After the jump the figure remains stationary, but in a slightly altered position.

In the employment of the tuning-fork means had to be devised for readily setting it in vibration: the use of the violin bow is obviously inconvenient; and this was soon replaced by a fork with the distance between the prongs less at the top than at the bottom, and which was started in the usual way by drawing a rod between the prongs. This mode has ultimately been replaced by the use of a short piece of soft iron, carried on an axis fixed between the prongs; when forced between the prongs the iron bar opens them to a sufficient extent, and on turning the axis rapidly through 90° the bar is placed parallel to the fork, which is left vibrating. This contrivance may be found useful for lecture purposes when large forks are used to show Lissajous' figures. It may be mentioned in passing that two forks without mirrors or lenses may readily be brought into unison or into some simple relation by attaching to one prong of each a thin piece of black paper with a fine slit. One fork is placed vertically and the other horizontally, a fixed lens being mounted between them so as to form an image of one slit on the other. Light is then passed through one slit; and on looking through the other a square dot of light is seen, which produces the Lissajous' figures when the forks are in motion. With a large fork the influence of the additional weight of the paper and attaching gum is imperceptible.

When a slit on a fork or reed is used together with the rotating cylinder, it is necessary to avoid parallax by throwing on the slit an image of the lines by means of a lens, and the observation is much facilitated by the employment of a second lens to view the slit. The reed is placed within a box capable of travelling on a fixed bed parallel to the cylinder, and the slit is soldered to the reed within the box; the latter is pierced by two holes closed with lenses. Some difficulty was at first experienced with the harmonium reed, which was placed in a box small enough to be readily movable along the slide, and to which the air had to be led by a flexible tube. The reed requiring a large quantity of air at low pressure to cause it to vibrate properly, it was not possible, without the use of large bellows and wide conducting-tubes, to produce the desired effect; it also appears essential that the air in contact with the reed should be contained in a chest of considerable dimensions, to permit of sufficient compression of the air when the tongue nearly closes the orifice. After numerous failures, a method, which we believe to be novel in this application, was found quite efficacious, namely, the employment of the principle of the injector or jet-pump. To the box is fixed a wide brass tube, with a considerable orifice in the side; at the end of the tube away from the box is fitted by a cork a glass tube, terminating in a narrow jet 1½ millim. in diameter; when air is forced through the jet at a pressure about equal to that of a column of water 20 or 25 centims. in height the reed vibrates perfectly, the mean pressure of the air in the box being equal to that of a column of water about 1½ millim. high.

Instead of, or together with, the graduated scale along which the box containing the reed or tuning-fork slides, a scale may be placed on a thin rule close to the cylinder, the numbers on which are visible simultaneously with the figures produced by the vibrating slit. This may be of some advantage when it is necessary to observe rapid changes of velocity. By drawing circles round the cylinder corresponding to the divisions of the scale, any possible error from slight shifting of the scale is obviated. With this arrangement it is not even necessary that the reed or tuning-fork should be near the rotating cylinder; for if the slit is placed at the focus of the object-glass of a telescope the observations can be made from a considerable distance; this may be useful when a shaft in an inaccessible situation is the subject of experiment.

It seemed advisable to determine the effect of alterations of temperature on the period of the fork, which might be so great as to render some correction necessary. For this purpose two equal forks provided with mirrors, and making about 60 vibrations a second, were enclosed in wooden boxes containing thermometers, which were placed between the prongs. One of the forks was fixed in a vertical position and the other horizontally, the box containing the latter having its lower side replaced by two pieces of sheet zinc, separated by a layer of air about half an inch in thickness. By the flame of a small gas-burner the temperature of the air in the box could be raised to about 60° C. At the side of each box was a hole opposite to the mirror, and closed by a plate of glass. The Lissajous' figure obtained by the reflection of a point of light in the two mirrors was observed in a small telescope; and the time required for the passage of the figure through a whole cycle was determined when the thermometers indicated different temperatures. There was some doubt whether the thermometers gave a true indication of the temperatures of the forks; but in the series of experiments giving the most concordant results the heat was applied near one end of the box, and at the greatest possible distance from the thermometer, in order to set up convection currents to equalize the temperature of the air and fork as completely as possible. It would be no doubt preferable to introduce into each box a small fan to keep the air in continuous motion; in all cases the temperature was changed very slowly, and a considerable time allowed to elapse between two observations. In the last series of nine observations the differences between the temperatures of the two forks varied from 9° to 27° C., and from the observed results the increase of the period of the heated fork for 1° C. was calculated. The mean loss per minute for 1° C. amounted to ·4013 vibration in 3600; the minimum number being ·3921, and the maximum ·4043. That is to say, a fork giving 3600 vibrations a minute at the ordinary temperature, would give 3599·6 when the temperature is raised 1°—a loss of about ·011 per cent. So that if a determination of velocity were made at a temperature 20° above that at which the fork is giving exactly 3600 vibrations per minute, a correction of ·22 per cent. would have to be deducted from the number obtained.

There is some difficulty in determining the exact number of vibrations of a large fork; the difficulty, however, disappears when a graduated cylinder or disk is driven with a constant velocity, and the number of vibrations per second calculated from the form of the figure produced by the fork. This method has been employed with the forks at present in use, and by proper arrangements it can be applied to the determination of the numbers of vibrations produced by musical forks; but up to the present we have failed in producing by means of clock-work a rotation which is sufficiently constant, our most successful results having been obtained with a six-horse power Corliss engine, with a heavy fly-wheel, in the workshop of the College.

Some experiments which promise well have been made with rotating disks on the principle of the thaumatrope. By using a rotating disk with slits, and viewing through them another disk on which appropriate figures or symbols are marked, the velocity of one can be determined if that of the other is known. This principle may possibly be applied with advantage to determine the relative velocity of two machines, such as the twin screws of ships.

Without pretending that the method described in this paper for determining the velocity of rotation will be useful on an extensive scale, we hope that it may be applicable in some cases of investigation where accurate observations can be rapidly made without any complex apparatus or difficult manipulation. It has the advantage that it can be applied directly to a machine without the intervention of any gearing, the mere attaching of a piece of paper to a shaft being all that is necessary. It cannot, by giving the machine more work to do, produce any effect on its rate; and by the impossibility of slip, it must give accurate results if the paper is properly mounted in the first instance and the observations are properly made.

Being an optical method for investigating rotation, we suggest cycloscope as a name for the instrument.

April 26, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

1. "On the Nature and Origin of the Beds of Chert in the Upper Carboniferous Limestones of Ireland." By Prof. EDWARD HULL, M.A., F.R.S., Director of the Geological Survey of Ireland. With "Chemical Notes," by E. T. HARDMAN, F.C.S., of the Geological Survey of Ireland. Received March 16, 1877.

(Abstract.)

After reviewing what had been published by previous authors on the origin of chert-beds, and showing that much remained to be done in this department of petrology, the author proceeded to describe the geological

position of the principal cherty zone of the Carboniferous Limestone of Ireland, showing that, while bands of chert occur at intervals throughout this formation, the highest beds immediately under "The Yoredale Shales" are especially rich in chert, and are frequently entirely replaced by this mineral. In these beds coralline, crinoidal, and other marine forms were frequently to be recognized by the naked eye. Thin slices for microscopic examination, taken from various localities, extending from Sligo to Carlow, also showed that even the most dense and compact masses of chert exhibit, under favourable circumstances, forms belonging to those marine animals (such as corals, crinoids, foraminifera, and occasionally mollusks) which build their shells or skeletons of carbonate of lime rather than of silica. The siliceous paste in which these forms are enclosed was found to be in a gelatinous state; and the forms were only to be distinguished by difference in depth of shade from the paste, the shells or skeletons having disappeared. The chemical analyses of these specimens by Mr. E. T. Hardman, F.C.S., tended to show that the chert-beds contain various proportions of carbonate of lime as well as other minerals, so that a gradation from siliceous limestone into pure chert might be traced.

From a review of the whole circumstances, it appeared that the origin of the chert-beds was to be attributed to the replacement of the original limestone or calcareous "ooze," due to organic agency, by silica, and that the rock is truly a pseudomorph, a view held by several observers.

The manner in which this replacement had been brought about was then touched upon. It was shown that there was reason for believing that at the close of the period during which the Carboniferous Limestone was formed over the area of Central Ireland, the sea-bed was elevated, so as to be covered by the waters of a shallow sea, exposed to the sun's rays, and of a warmer temperature than when at a greater depth. The waters appear to have been charged with a more than usual supply of silica in solution, derived (as Mr. Hardman suggests) from the surrounding lands, formed, for the most part, of highly siliceous materials. As silica is less soluble than carbonate of lime, chemical replacement would naturally take place, the carbonate of lime being dissolved out and its place taken by the silica. The warm condition of the sea-water, its exposure to sunlight, the porous character of the coralline, crinoidal, and other forms, and the soft and "oozy" condition of the foraminiferal mud would give easy access to the sea-waters, and a process of silicification would take place analogous to that described by Dr. Martin Duncan, F.R.S., as having occurred in the West Indies.

The paper was accompanied by chemical analyses and photographic figures of some of the thin slices, slightly magnified.

II. "Researches on Emeralds and Beryls.—Part II. On some of the Processes employed in the Analysis of Emeralds and Beryls." By GREVILLE WILLIAMS, F.R.S. Received April 5, 1877.

While analyzing the beryl "A" from Ireland, frequently mentioned in the first part of this investigation*, so many unexpected phenomena presented themselves that it was found necessary to study carefully all the processes which have been published for the separation of glucina from alumina. On consulting the numerous papers on the subject, it became apparent that a wide difference of opinion existed among chemists as to the best method of working. Among the eleven or twelve methods which have been proposed for the purpose, there are three which have been especially employed in the most important researches. I place these below, and under each heading will be found the names of some of the chemists who have used the process, the first in each case being that of the inventor.

Carbonate of ammonium.	Hydrate of potassium.	Chloride of ammonium.
Vauquelin..1798.	Vauquelin ....1798.	Berzelius..1812.
Klaproth ..1801.	C. G. Gmelin..1840.	Weeren ..1854.
Lewy......1857.	Awdejew ....1843.	
Hofmeister.1859.	Damour ......1843.	
	Ebelmen......1848.	

The following pages are devoted to a study of the first of these, namely, the carbonate-of-ammonium process.

The first chemist to take advantage of the solubility of glucina in carbonate of ammonium, as a means of separating it from alumina, was Vauquelin, who, in his earliest memoir on the beryl, in which he announces the discovery of glucina†, mentions the fact of its solubility in a solution of carbonate of ammonium; but at that time he separated the two earths by converting the alumina into alum and removing the latter by crystallization. The small proportion of alumina remaining was separated by taking advantage of its solubility in a solution of hydrate of potassium even when heated, whereas, under these circumstances, glucina is precipitated—Vauquelin thus distinctly being the first to use the process now called Gmelin's. In Vauquelin's second memoir, published in the same volume of the 'Annales de Chimie,' he employed carbonate of ammonium for the purpose of separating the two earths. The various chemists who followed this process for many years after Vauquelin's time appear to have used it in its simplest form, the directions given by Rose[*] (whose name is generally, but incorrectly, given to the method) being merely to use a sufficiently large quantity of the carbonate. He admits that a little alumina is dissolved at the same time. Wöhler[†] directs the solution of the two earths to be dropped very gradually with constant stirring into an excess of a warm concentrated solution of carbonate of ammonium, which precipitates the alumina and dissolves the glucina.

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* Proc. Royal Society, No. 145, 1873.

† Ann. de Ch. xxvi. 1798. The name of this journal has since been modified.

That one extraction with carbonate of ammonium is quite insufficient will be seen from the following experiments and considerations.

The mixed earths (glucina and alumina) from 2·3217 grammes of the beryl A were precipitated by ammonium hydrate and ammonium sulphide, and allowed to stand all night in a large excess of carbonate of ammonium; the resulting glucina weighed 0·1806 gramme, or only 7·78 per cent. on the beryl, whereas three more extractions raised the percentage of glucina to 11·55[‡].

Again, a mixture was taken of 0·2640 gramme of glucina and 0·8520 of alumina, the percentage being:—

Alumina	76·34
Glucina	23·66
	100·00

The mixture was dissolved in hydrochloric acid, neutralized with carbonate of ammonium, and digested for twenty-four hours with 95 cub. centims. of a saturated solution of carbonate of ammonium (sp. gr. 1·080 at 15° C.), or 8·5 cub. centims. for each decigramme of the mixed earths. On acidifying the filtered solution with hydrochloric acid, and precipitating with ammonium hydrate and ammonium sulphide, only 12·75 per cent. of glucina was obtained instead of 23·66.

But it might be said that the deficiency in the glucina in the experiment quoted arose from the use of an insufficient quantity of carbonate-of-ammonium solution. To settle this point definitely the following experiments were made:—

Experiment I.—0·1960 gramme of glucina and 0·1812 gramme of alumina were dissolved in hydrochloric acid, neutralized by ammonia, and treated with 100 cub. centims. of carbonate-of-ammonium solution. This is at the rate of about 27 cub. centims. for each decigramme of the mixed earths. A temperature of 65° C. was kept up for five and a half hours, the exit-tube of the flask being closed with a mercurial valve. It was found that a pressure of three inches of mercury was sufficient to prevent the escape of vapour at the temperature stated. The solution was allowed to stand in the cold for fifteen hours longer, and was then filtered. The alumina, after being well washed, dried, and ignited, weighed 0·2089 gramme.

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[*] Rose's Chem. Anal. Translated by Normandy, ii. p. 58. 1849.

[†] Handbook of Inorganic Analysis, 1854.

[‡] I have much pleasure in acknowledging the skill and accuracy with which some of these fractional extractions were performed by my friend Mr. Raphael Meldola, who was my assistant at that time.

	Composition of mixture.	Found after one extraction at 65° C.
Glucina	51·96	
Alumina	48·04	55·38
	100·00	

or 7·34 per cent. in excess.

It was now determined to make one more experiment with double the quantity of carbonate of ammonium used in the last experiment.

Experiment II.—0·1945 gramme of glucina and 0·1862 gramme of alumina were dissolved and treated in the cold for twenty hours with 200 cub. centims. of the carbonate-of-ammonium solution. This was at the rate of 52 cub. centims. for each decigramme of the mixed earths.

	Composition of mixture.	Found after one extraction.
Glucina	51·09	
Alumina	48·91	55·69
	100·00	

or 6·78 per cent. in excess.

It is not surprising, therefore, after seeing the results of one extraction under such varied conditions, that Damour[*], who tried the carbonate-of-ammonium process in its original form, in his important investigation of chrysoberyl, rejected it in favour of Gmelin's process, on the ground that it failed to extract more than 12 out of the 19·75 per cent. of glucina contained in that mineral. Damour, however, obtained more than 1 per cent. less glucina in proportion than was obtained in the first experiment quoted above. The two results are, nevertheless, remarkably close, and confirm the assertion that one treatment with carbonate of ammonium conducted in the manner described is quite inadequate to extract all the glucina from a mixture of the two earths.

Joy[†], in opposition to Damour, condemns Gmelin's process as giving in his hands too low a percentage of glucina in spite of every precaution. Weeren[‡], who followed Berzelius's method, condemns the carbonate-of-ammonium process on exactly the opposite ground to Damour, namely, because although alumina *per se* is, he says, insoluble in carbonate of ammonium, it is soluble in the presence of glucina, and thus gives too high a number for the latter. I admit that alumina, under certain cir-

[*] Ann. Ch. Phys. [3] vii. p. 173 (1843).

[†] American Journal of Science and Arts, xxxvi. p. 83 (1863). This interesting paper contains an invaluable list of memoirs on all subjects connected with the minerals which contain glucina.

[‡] Pogg. Ann. xcii. p. 91 (1854).

cumstances, dissolves in presence of glucina; but I shall show further on that the fact does not prevent fairly accurate results being obtained if certain precautions be observed. This tendency of the earths, when mixed, to modify each other's reactions is seen also in their behaviour with carbonate of barium. I have, however, never found the carbonate-of-ammonium process to give too high a number for the glucina, for reasons which are fully discussed further on.

Hofmeister[*], who has made more important and elaborate observations on the carbonate-of-ammonium process than any other chemist, condemns Gmelin's process on the ground that in the treatment with potassium hydrate alumina accompanies the glucina[†]. He also endorses the statement of Weeren and others, that alumina is soluble in carbonate of ammonium in presence of glucina, but shows that this circumstance is not fatal to the process if a systematic fractional treatment is employed.

Hofmeister admits that his method is not easy to describe; and, in fact, it is so operose that the reader must be referred to the original memoir for the details. There can be no doubt that when carefully followed it affords reliable results. I shall endeavour to show further on that the carbonate-of-ammonium process is capable of yielding accurate numbers when employed in a simpler manner.

From what has been said, it will readily be seen that the two processes I have alluded to are condemned by different experimenters on diametrically opposite grounds; and it seems to me that, as is usually the case under such circumstances, the whole question resolves itself into one of manipulation. This being assumed, it became necessary (in order to ascertain the causes of the conflicting statements of the numerous chemists who have subjected emeralds and beryls to analysis) to make a rigorous examination of the behaviour of glucina with solution of carbonate of ammonium. This has entailed much work, as I have taken most of the statements of the chemists who have worked on the subject and examined them carefully and repeatedly by experiment.

The following are the principal questions or problems which I have endeavoured to solve:—

1. Is glucina permanently soluble in a solution of carbonate of ammonium?
2. Does glucina confer its solubility on alumina? or does alumina confer its insolubility on glucina?
3. With what amount of accuracy can a mixture of glucina and alumina be separated by means of carbonate of ammonium?
4. How do solutions of glucina and alumina behave with carbonate of barium?

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[*] Journ. für prakt. Chem. lxxvi. p. 1.

[†] I have confirmed this observation.

1. *Is Glucina permanently soluble in a solution of Carbonate of Ammonium?*

This question is of paramount importance, because if glucina once dissolved in carbonate of ammonium is liable to assume an insoluble condition and separate from the solution, it introduces a complication into the process, the effect of which it is difficult to estimate. The only chemist, as far as I am aware, who asserts that glucina separates out under the circumstances named is Joy*, who states that "If the solution be kept longer than ten days a precipitate of carbonate of glucina will begin to form, and at the expiration of sixteen days 15 per cent. less of the original amount will go (*sic*) into solution." This assertion of Joy's caused me some perplexity at first, as I had during many years repeatedly dissolved glucina in solution of carbonate of ammonium, but I had never observed a deposit to take place *unless I had reason to suspect the presence of alumina.* I therefore resolved to submit pure glucina and mixtures of that earth with alumina to repeated quantitative experiments, with the view of deciding the question.

Experiment I.—0·6943 gramme of glucina, which had already been partially purified by solution in carbonate of ammonium, was dissolved in hydrochloric acid; ammonium hydrate was then dropped in until the solution was very nearly neutral, and 138 cub. centims. of solution of carbonate of ammonium were added†. At first nearly every thing was dissolved, but a precipitate soon commenced to form. This is quite characteristic of the presence of alumina. The solution was filtered next day, and the precipitate was collected, washed, dried, and burnt with all the usual precautions. The weight of the alumina was 0·0058 gramme, or 0·84 per cent. on the glucina taken. The solution was put aside in a well-stoppered bottle, as I intended to filter it in sixteen days; but as much more than that time passed without a precipitate forming, it was put carefully away for *three years.* At the end of this time the bottle, on being shaken, showed a barely perceptible trace of deposit; nevertheless it was filtered off with every precaution and was found to weigh 0·0038 gramme, or 0·55 per cent.; and this was probably partly alumina and partly silicate, arising from a slight decomposition which had taken place of the glass of the bottle.

Experiment II.—0·2140 gramme of the glucina which had remained three years in solution was next experimented on. The earth was dissolved and treated as before with 50 cub. centims. of carbonate-of-ammonium solution. In ten minutes after being well agitated all but a few imponderable flocks were dissolved. In fifty minutes they were scarcely visible. Two days after, they had totally disappeared. In 16 days, the period at which Joy says 15 per cent. less of the glucina will remain in solution, the liquid was filtered and yielded 0·0023 gramme of precipitate, or 1·07 per cent., a quantity which might partly arise from insufficient washing, partly from a variation from the mean weight in the filter ash, and also from a trace of alumina.

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* *Loc. cit.* p. 89.

† The solution of carbonate of ammonium used in these experiments was always of the same strength, *i. e.* a cold saturated solution, sp. gr. 1·080.

Experiment III.—0·1603 gramme of glucina, known to contain a little alumina, was dissolved in hydrochloric acid, neutralized as usual, and treated with 50 cub. centims. of solution of carbonate of ammonium. The residue of alumina weighed 0·0071. In two days 0·0067 gramme more was obtained. On filtering 16 days afterwards only 0·0017 gramme, or 1·06 per cent., was obtained. In one year after that time the solution was again filtered, the deposit weighed 0·0012 gramme, or 0·75 per cent., being only one third of the weight of the ash of one filter paper.

Experiment IV.—0·3846 gramme of *pure* glucina was treated in the usual manner with 80 cub. centims. of carbonate-of-ammonium solution. The whole dissolved perfectly in a few minutes. The solution was kept for three weeks and then filtered. The precipitate weighed 0·0003 gramme, or 0·08 per cent., after deducting the filter ash. If it were necessary these examples could be multiplied; but they seem amply sufficient to enable an answer to be given to the question at the head of this section. We may therefore affirm unhesitatingly that *one decigramme of pure glucina is permanently soluble in* 25 *cub. centims. of a saturated solution of carbonate of ammonium.*

It was necessary, in order to make these experiments complete, to ascertain whether great variations in temperature influenced the solubility of glucina in solution of carbonate of ammonium. That glucina was permanently soluble at about 15° C. there remained no doubt; but there might still be a tendency in glucina to assume an insoluble form at higher temperatures. To determine this question it was essential to use a temperature many degrees higher than that ever reached by the atmosphere. For this purpose 4 decigrammes of pure glucina were dissolved in the usual way in 100 cub. centims. of solution of carbonate of ammonium. 15 cub. centims. of this solution were transferred to a glass tube, which was then sealed and heated to 100° for two days. *The solution remained perfectly clear.*

2. *Does Glucina confer its solubility on Alumina? or does Alumina confer its insolubility on Glucina?*

It is very easy to come to an erroneous conclusion on this point, because if a mixture of the two earths in solution of carbonate of ammonium be filtered *at once* a certain amount of alumina will, in that case, pass through the filter; but it settles out in a short time, and, if glucina be present, it will be accompanied by some of that earth.

The following experiment was made to affix a quantitative value to the amount of alumina soluble in carbonate of ammonium when no glucina was present to influence its solubility.

0·5000 gramme of pure alumina was dissolved in hydrochloric acid and precipitated (after neutralization with carbonate of ammonium) with 100 cub. centims. of solution of the carbonate. The whole was thrown on a filter at once. The contents of the filter were washed, dried, and ignited. The alumina which had precipitated at once weighed 0·4702 gramme. The filtrate, on standing twenty-four hours, had yielded a deposit weighing 0·0281 gramme. These numbers, reduced to percentages, are as follows:—

Alumina precipitated at once......	94·04
„ coming down afterwards..	5·62
Loss....	·34
	100·00

It was considered that the question whether glucina confers its solubility on alumina would be best answered by treating with carbonate of ammonium a mixture containing a great excess of glucina.

Experiment I.—A mixture was therefore made of 0·2500 gramme of glucina and 0·0250 gramme of alumina. It was dissolved in hydrochloric acid, and the excess of acid was removed by evaporation; 50 cub. centims. of carbonate-of-ammonium solution were then added. The precipitate at first formed *entirely redissolved* in a few minutes, but the alumina commenced to deposit in about fifteen minutes afterwards. The mixture was allowed to stand for twenty-four hours and was then filtered. The precipitate of alumina containing glucina weighed 0·0323 gramme. The glucina recovered from the solution weighed 0·2407 gramme; or, per cent.:—

	Composition of mixture.	Found after one extraction.
Glucina	90·91	87·53
Alumina	9·09	11·75
	100·00	99·28

The glucina, although in such great excess, had therefore, under the circumstances indicated, not conferred its solubility on the alumina; but, on the contrary, the alumina had conferred its insolubility on the glucina.

Experiment II.—The alumina obtained in the manner described was redissolved, reprecipitated, and again treated with 25 cub. centims. of carbonate-of-ammonium solution for twenty-four hours. The glucina had now increased to 0·2432, and the alumina diminished to 0·0298. The composition of the mixture, as found after two extractions, was therefore as follows:—

	Found after two extractions.
Glucina................	88·44
Alumina	10·84
	99·27

After two extractions the alumina therefore still retained nearly 2 per cent. of glucina.

Experiment III.—Although these results seemed conclusive as far as they went, I resolved to repeat the experiment with a mixture containing a still greater excess of glucina, because it appeared especially important in an inquiry of this kind to know the behaviour towards a solution of carbonate of ammonium of glucina containing a comparatively small proportion of alumina. For this purpose a mixture was taken of 1 gramme of glucina and 3 centigrammes of alumina; it was dissolved as usual, precipitated in the form of carbonates of the earths, and treated with 100 cub. centims. of solution of carbonate of ammonium. The earths dissolved entirely at first; but, small as the quantity of alumina was, the solution began to get turbid in about ten minutes. The mixture was left in the cold for twenty hours. The residue of alumina containing glucina was then filtered off, treated in the usual manner, and weighed. The glucina recovered from the solution weighed 0·9846 gramme.

	Composition of mixture.		Found after one extraction.
Glucina ..	97·09	....	95·59
Alumina ..	2·91	....	4·16
	100·00		99·75

Alumina, therefore, even when mixed with glucina to the small extent of 3 per cent., renders some of the latter insoluble; this, then, is a distinct answer to the question at the head of the section.

3. *With what amount of accuracy can a mixture of Glucina and Alumina be separated by means of Carbonate of Ammonium?*

The fact that alumina confers its insolubility on glucina is the cause of the difficulties that have been found in the separation of the two earths. We have seen that, under the conditions indicated, one treatment with carbonate of ammonium is insufficient to dissolve the glucina out from such a mixture. I wish, however, to guard myself from appearing to express the opinion that it would be impossible, by a modification of the process, to effect the separation at one operation, as, in fact, I am engaged at the present moment in an attempt to solve that problem.

In order to ascertain the number of times that it would be necessary to treat the insoluble residue with carbonate of ammonium, in order to extract all the glucina, the following experiments were made.

Experiment I.—A mixture was taken of 0·5000 gramme of glucina and the same amount of alumina; after three treatments for forty-eight hours each with carbonate of ammonium the following results were obtained. The residue of alumina weighed 0·5107 gramme.

No. of extractions.	Glucina obtained.
I.	0·3896
II.	0·0739
III.	0·0203
	0·4838

	Composition of mixture.	Obtained in three treatments.
Glucina	50·00	48·38
Alumina	50·00	51·07
	100·00	99·45

Experiment II.—Upon a similar mixture the following modification of the process was tried. The mixture was dissolved in hydrochloric acid, the excess of acid was neutralized with ammonia, and 200 cub. centims. of a *warm* solution of ammonium carbonate were added. The solution was stirred briskly for five minutes, and then filtered off rapidly. The precipitate was only washed moderately so as not to dilute the filtrate too much. The filtrate was allowed to stand for twenty-four hours, the deposit was filtered off, added to the first residue, dissolved with it in hydrochloric acid, and again treated twice with 100 cub. centims. of carbonate-of-ammonium solution in the cold for twenty-four hours each time. The alumina weighed 0·5201 gramme.

No. of extractions.	Glucina obtained.
I.	0·4450
II.	0·0163
III.	0·0161
	0·4774

	Composition of mixture.	Obtained in three treatments.
Glucina	50·00	47·74
Alumina	50·00	52·01
	100·00	99·75

Experiment III.—A mixture was then prepared of 0·2640 gramme of glucina and 0·8520 gramme of alumina. It was dissolved in hydrochloric acid, neutralized with carbonate of ammonium, and digested with 95 cub. centims. of the carbonate-of-ammonium solution for twenty-four hours; it was stirred occasionally. The solution was then filtered, the residue dissolved in hydrochloric acid, and, after neutralizing, was treated with 100 cub. centims. of a cold solution of carbonate of ammonium, and allowed to stand twelve hours. The glucina was separated in the usual manner. This mode of proceeding was repeated seven times, with the results given in the annexed Table.

No. of extractions.	Glucina obtained (per cent.).
I.	12·75
II.	3·24
III.	0·61
IV.	3·74
V.	1·26
VI.	0·56
VII.	1·13
	23·29

	Composition of mixture.	Obtained in seven treatments.
Glucina	23·66	23·29
Alumina	76·34	
	100·00	

The alumina was not estimated in this experiment.

The answer, then, to the question at the head of the section is, that results to within half a per cent. of the truth can be obtained by means of the carbonate-of-ammonium process if a sufficient number of extractions be made.

4. *How do solutions of Glucina and Alumina behave with Carbonate of Barium?*

As it frequently happened, during my analyses of emeralds and beryls, that it would have been a great convenience to be able to precipitate the glucina, alumina, and iron together by means of carbonate of barium, it became necessary to examine the behaviour of the two earths with that reagent, especially since, as happens so frequently in the literature relating to glucina, there is a difference of opinion among chemists upon the subject. Rose [*] states that solutions of glucina are not precipitated in the cold; Fresenius [†] that carbonate of barium precipitates glucina completely upon cold digestion. This is at variance with the observations of Awdejew [‡], who states that only partial precipitation takes place in the cold, but that the precipitation is almost complete on boiling. Joy [§] endorses Weeren's statement, that both glucina and alumina are precipitated, but does not enter into details. Ordway [||] states that a solution of nitrate of glucina is only partially precipitated in the cold by carbonate of barium, a soluble basic nitrate remaining in solution, but that the precipitation is completed on boiling. To study this subject quantitatively I made the following experiments.

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[*] Handb. d. chem. Anal. ii. p. 61.

[†] Qualitative Analysis, 8th edit. p. 103.

[‡] Pogg. Ann. lvi. p. 101.

[§] *Loc. cit.*

[||] Sill. Am. J. [2] xxxi. p. 197.

Experiment I.—*Precipitation of Alumina in the cold by Carbonate of Barium.*

0·8034 gramme of pure alumina was dissolved in hydrochloric acid. The solution was nearly neutralized with carbonate of sodium, and an excess of carbonate of barium, made into a cream with water, was added. After standing twelve hours the precipitate was collected, washed and dissolved in hydrochloric acid. The solution was then boiled and precipitated with an excess of sulphuric acid. The sulphate of barium was filtered off with the usual precautions, and the filtrate was precipitated by ammonia. The precipitate was thoroughly washed, dried, and ignited; it weighed 0·8005 gramme, or 99·64 per cent. on the original alumina. Alumina is therefore completely precipitated in the cold by carbonate of barium.

Experiment II.—*Precipitation of Glucina in the cold by Carbonate of Barium.*

0·5175 gramme of pure glucina were dissolved in hydrochloric acid, and treated precisely as the alumina had been in the last experiment. The precipitate weighed 0·1070 gramme, or 20·68 per cent. on the original glucina. This result, therefore, confirms the observation of Awdejew, that glucina is only imperfectly precipitated by carbonate of barium in the cold, and, under the circumstances indicated, behaves like the nitrate in Ordway's experiments.

Experiment III.—*Precipitation of a mixture of Glucina and Alumina in the cold by Carbonate of Barium.*

0·2096 gramme of pure alumina and 0·2055 of pure glucina were dissolved in hydrochloric acid and treated precisely like the alumina in Experiment I. The precipitate of the mixed earths weighed 0·3874 gramme, or 93·33 per cent. on the original weight. Alumina, therefore, in this case, as in the experiments with carbonate of ammonium, communicates much of its insolubility to the glucina.

Assuming only the portion precipitated by carbonate of barium to be accounted for in the analysis of a beryl containing 28·89 per cent. of the mixed earths, only 26·96 would be obtained, the loss being 6·67 per cent., which, as we have seen, would fall chiefly upon the glucina, if the operation were conducted in the cold. This loss is, however, variable, and appears to depend, to some extent, upon the relative proportions of the two earths.

III. "On Repulsion resulting from Radiation.—Preliminary Note on the Otheoscope." By William Crookes, F.R.S. &c. Received April 23, 1877.

I communicated to the Royal Society in November last an account of some radiometers which I had made with the object of putting to experimental proof the "molecular pressure" theory of the repulsion resulting from radiation. Continuing these researches, I have constructed other instruments, in which a movable fly is caused to rotate by the molecular pressure generated on fixed parts of the apparatus.

In the radiometer, the surface which produces the molecular disturbance is mounted on a fly, and is driven backwards by the excess of pressure between it and the sides of the containing vessel. Regarding the radiometer as a heat-engine, it is seen to be imperfect in many respects. The black or driving surface, corresponding to the heater of the engine, being also part of the moving fly, is restricted as to weight, material, and area of surface. It must be of the lightest possible construction, or friction will greatly interfere with its movement; it must not expose much surface, or it will be too heavy; and it must be a very bad conductor of heat, so as to retain the excess of pressure on one side. Again, the part corresponding to the cooler of the engine (the side of the glass bulb) admits of but little modification. It must almost necessarily be of glass, by no means the best material for the purpose; it is obliged to be of one particular shape; and it cannot be brought very near the driving surface.

A perfect instrument would be one in which the *heater* was stationary; it might then be of the most suitable material, of sufficient area of surface, and of the most efficient shape, irrespective of weight. The *cooler* should be the part which moves; it should be as close as possible to the heater, and of the best size, shape, and weight for utilizing the force impinging on it. By having the driving surface of large size, and making it of a good conductor of heat, such as silver, gold, or copper, a very faint amount of incident radiation suffices to produce motion. The black surface acts as if a molecular* wind were blowing from it, principally in a direction normal to the surface. This wind blows away whatever easily movable body happens to be in front of it, irrespective of colour, shape, or material; and in its capability of deflection from one surface to another, its arrest by solid bodies, and its tangential action, it behaves in most respects like an actual wind.

Whilst the radiometer admits of but few modifications, such an instrument as the one here sketched out is capable of an almost endless variety of forms; and as it is essentially different in its construction and mode of action to the radiometer, I propose to identify it by a distinctive name, and call it the Otheoscope (*ὠθέω*, I propel).

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* *Molecular*, not *molar*. There is no wind in the sense of an actual transference of air from one place to another. This molecular movement may be compared to the movement of the gases when water is decomposed by an electric current. In the water connecting the two poles there is no apparent movement, although eight times as much matter is passing one way as the other.

The glass bulb is an essential portion of the machinery of the radiometer, without which the fly would not move; but in the otheoscope the glass vessel simply acts as a preserver of the requsite amount of rarefaction. Carry a radiometer to a point in space where the atmospheric pressure is equal to, say, one millimetre of mercury, and remove the glass bulb; the fly will not move, however strong the incident radiation. But place the otheoscope in the same conditions, and it will move as well without the case as with it.

In the preliminary note already referred to[*], I described a piece of apparatus by which I was able to measure the thickness of the layer of molecular pressure generated when radiation impinged on a blackened surface at any degree of exhaustion. At the ordinary density of the atmosphere the existence of this molecular disturbance was detected several millimetres off, and its intensity increased largely as the generating surface and movable plate were brought closer together. It would be possible, therefore, to construct an otheoscope in which no rarefaction or containing vessel was necessary, but in which motion would take place in air at the normal density[†]. Such a heat-engine would probably work very well in sunlight.

Aided by the mechanical dexterity of my assistant, Mr. C. H. Gimingham, I have constructed several varieties of otheoscope. These will be exhibited at the Soirée of the Royal Society on Wednesday next, as illustrations of the very beautiful manner in which, at this stage of my investigations, theory and experiment proceed hand in hand, alternately assisting each other, and enlarging our knowledge of these laws of molecular movement which constitute a key to the relations of force and matter.

The following is a list of the otheoscopes I have already made, together with some new experimental radiometers, which will be exhibited for the first time on Wednesday:—

1. *Otheoscope.*—A four-armed fly, carrying four vanes of thin clear mica, is mounted like a radiometer in an exhausted glass bulb. At one side of the bulb a plate of mica blacked one side is fastened in a vertical plane, in such a position that each clear vane in rotating shall pass the plate, leaving a space between of about a millimetre. If a candle is brought near, and by means of a shade the light is allowed to fall only on the clear vanes, no motion is produced; but if the light shines on the black

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[*] Proc. Royal Soc. Nov. 16, 1876, p. 310.

[†] Since writing this I have constructed such an instrument. The movement takes place in the way I had anticipated.—W. C., April 26th, 1877.

plate, the fly instantly rotates as if a wind were issuing from this surface, and keeps on moving as long as the light is near.

2. *Otheoscope.*—A four-armed fly carries roasted mica vanes, and is mounted in an exhausted glass bulb like a radiometer. Fixed to the side of the bulb are three plates of clear mica, equidistant from each other in a vertical plane, but oblique to the axis. A candle brought near the fixed plates generates molecular pressure, which, falling obliquely on the fly, causes it to rotate.

3. *Otheoscope.*—A large horizontal disk revolving by the molecular disturbance on the surface of inclined metallic vanes, which are blacked on both sides in order to absorb the maximum amount of radiation.

4. *Otheoscope.*—Inclined aluminium vanes driven by the molecular disturbance from the fixed black mica disk below, blowing (so to speak) through them.

5. *Otheoscope.*—A large horizontal coloured disk of roasted mica, driven by inclined aluminium vanes placed underneath it.

6. *Otheoscope.*—A bright aluminium disk cut in segments, and each segment turned at an angle, driven by a similar one below of lampblacked silver.

7. *Radiometer.*—A vertical radiometer, made with eight disks of mica blacked on one side, and the whole suspended on a horizontal axis which works in two glass cups. The motion of the radiometer is assisted on each side by driving vanes of aluminium blacked on one side.

8. *Radiometer.*—A vertical turbine radiometer, the oval vanes of roasted mica blacked on one side.

9. *Radiometer.*—A spiral radiometer of roasted mica blacked on the upper side.

10. *Radiometer* of large size, showing great sensitiveness.

11. *Radiometer.*—A two-disk radiometer, the fly carrying roasted mica disks blacked on one side; in front of each black surface is fixed a large disk of thin clear mica. The molecular disturbance set up on the black surface, and streaming from it, is reflected in the opposite direction by the clear plate of mica, causing the fly to move abnormally, *i. e.* the black surface towards the light.

12. *Radiometer.*—A two-disk radiometer, the fly carrying roasted mica disks blacked on one side, similar to No. 11, but with a large clear disk on each side. The molecular disturbance, prevented from being reflected backwards by the second clear disk, is thus caused to expand itself in a vertical plane, the result being a total loss of sensitiveness.

13. *Radiometer.*—A two-disk, cup-shaped, aluminium radiometer, facing opposite ways; both sides bright. Exposed to a standard candle 3·5 inches off, the fly rotates continuously at the rate of one revolution in 3·37 seconds. A screen placed in front, so as to let the light shine only on the convex surface, produces repulsion of the latter, causing continuous rotation at the rate of one revolution in 7·5 seconds. When the

convex side is screened off, so as to let the light shine only on the concave, continuous rotation is produced at the rate of one revolution in 6·95 seconds, the concave side being apparently attracted. These experiments show that the repulsive action of radiation on the convex side is about equal to the attractive action of radiation on the concave side, and that the double speed with which the fly moves when no screen is interposed is the sum of the attractive and repulsive actions.

14. *Radiometer.*—A two-disk, cup-shaped, aluminium radiometer, lampblacked on the concave surfaces. In this instrument the usual action of light is reversed, rotation taking place, the bright convex side being repelled, and the black concave attracted. When the light shines only on the bright convex side, no movement is produced; but when it shines on the black concave side, this is attracted, producing rotation.

15. *Radiometer.*—A cup-shaped radiometer similar to the above, but having the convex surfaces black and the concave bright. Light shining on this instrument causes it to rotate rapidly, the convex black being repelled. No movement is produced on letting the light shine on the bright concave surface, but good rotation is produced when only the black convex surface is illuminated.

16. *Radiometer.*—A multiple-disk, cup-shaped, turbine radiometer, bright on both sides, working by the action of warm water below and the cooling effect of the air above.

17. *Radiometer.*—A four-armed, metallic radiometer with deep cups, bright on both sides.

18. *Radiometer.*—A four-armed radiometer, the vanes consisting of mica cups, bright on both sides.

19. *Radiometer.*—A four-armed radiometer having clear mica vanes, the direction of motion being determined by the angle formed by the mica vanes with the inner surface of the glass bulb.

IV. "On the Inferences to be drawn from the Appearance of Bright Lines in the Spectra of Irresolvable Nebulæ." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received April 26, 1877.

In a paper recently read before the Royal Society, Mr. Stone attempts to show that the fact that the spectra of some of the irresolvable nebulæ consist mainly of bright lines does not warrant the inference that these bodies are of a constitution different from our sun and the generality of the fixed stars, and consist mainly of glowing gas, so far, at least, as the light-giving portion of them is concerned.

Waiving for the present the objections which may be urged against Mr. Stone's reasoning, let us consider the question in the light of the results afforded by actual observation.

There are not found in the spectra of different nebulæ the differences of relative brightness of the bright lines and of the continuous spectrum which would be expected on Mr. Stone's hypothesis.

The star-clusters which are just within the resolving-power of the largest telescopes do not give, even faintly, a spectrum of bright lines.

The same bright lines appear to be common to all the nebulæ which give a bright-line spectrum. On Mr. Stone's view, differences in the constitution of the enclosing atmospheres of different star-groups would be probable.

On this point I may be permitted, perhaps, to add the following sentences from my paper "On the Spectra of some of the Nebulæ"*:—

"It is indeed *possible* that suns endowed with these peculiar conditions of luminosity (giving bright-line spectra) may exist, and that these bodies are clusters of such suns. There are, however, some considerations, especially in the case of planetary nebulæ, which are scarcely in accordance with the opinion that they are clusters of suns. Sir John Herschel remarks of one of this class, in reference to the absence of central condensation:—'Such an appearance would not be presented by a globular space uniformly filled with stars or luminous matter, which structure would necessarily give rise to an apparent increase of brightness towards the centre, in proportion to the thickness traversed by the visual ray. We might therefore be inclined to conclude its real constitution to be either that of a hollow spherical shell, or of a flat disk presented to us (by a highly improbable coincidence) in a plane precisely perpendicular to the visual ray'†.

"This absence of condensation admits of explanation without recourse to the supposition of a shell or flat disk, if we consider them to be masses of glowing gas. For supposing, as we probably must do, that the whole mass of the gas is luminous, yet it would follow, by the law which results from the investigations of Kirchhoff, that the light emitted by the portions of gas beyond the surface visible to us would be in great measure, if not wholly, absorbed by the portion of gas through which it would have to pass; and for this reason there would be presented to us a luminous surface only"‡.

It appears, therefore, that the results of observation do not accord well with Mr. Stone's theory.

But the theory itself appears open to grave objections. It is obvious (and was strongly insisted upon by Prof. Stokes in remarks made when the paper was read) that in a star-cluster in which the stars are surrounded by self-luminous atmospheres, the *proportion between the sum total* of the light from the stars and the light from the atmospheres will be independent of the distance of the cluster from us. Unless, then, we suppose that the light received from our own sun is but a fraction of the total light received from a supposed atmosphere of enormous extent surrounding him (a supposition which needs only to be stated to be rejected), instead of constituting the main portion of the total light, it follows that the total light received from a distant cluster formed of stars at all resembling our own sun must mainly come from the stars themselves. If, then, it be true, as it undoubtedly is, and as Mr. Stone has urged, that at a sufficient distance the light from any individual star is insignificant, while that from the cluster as a whole (both stars and atmospheres) is not, this can only be by the distance being so great that the small but finite solid angle subtended by a small portion of the slit employed in the investigation is nevertheless sufficient to take in a considerable number of the stars; and if this be admitted, Mr. Stone's reasoning falls to the ground.

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* Phil. Trans. 1864, p. 443.

† 'Outlines of Astronomy,' 7th edit. p. 646.

‡ See also Sir William Herschel, Phil. Trans. 1811, pp. 314, 315.

Presents received, April 12, 1877.

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"On the Temperature of the Human Body in Health." By SYDNEY RINGER, M.D., Professor of Therapeutics at University College, London, and the late ANDREW P. STUART, M.R.C.S. Communicated by Dr. SHARPEY, F.R.S. Received January 19, 1877*.

PART I.

These investigations were made in order to learn how the temperature of the body comports itself in health; since to every one it must be obvious that without such knowledge it is impossible to determine with any approach to certainty what variations in the temperature are to be accepted as indications of disease.

It is necessary to state here some particulars of the manner these investigations were conducted, and of the precautions taken to avoid error.

The subjects of our observations were confined to bed during the times when the investigations, continued on some occasions for several days, were made. They were well covered with bed-clothes, and, indeed, as far as possible throughout the time they remained in bed, were kept under the same conditions, so that any alterations in their temperature could not be ascribed to any exposure, or to any variation in the temperature of the room, or to other accidental and preventible causes.

The thermometer was generally placed in the axilla, though on some occasions the temperature of the mouth under the tongue and of the rectum was noted at the same time. These observations are sufficiently numerous to enable us to say that, due care being taken and sufficient time allowed, the temperature of the axilla is always identical with that of the mouth and with that of the rectum, four to six inches above its termination.

A non-registering thermometer was retained in the axilla and read *in situ* until the completion of each series of these observations; and the temperature was noted hourly, or nearly so, from 9 A.M. to a late hour in the evening, usually to 12 P.M. On some days hourly observations were made during both night and day. The time of taking food and the kind and quantity were always accurately recorded.

It is well to mention that some of the persons who were the subjects of this investigation were convalescents from various diseases, but had fairly recovered, and might be considered to be in tolerably good health.

That the subsequent remarks may be more clearly understood, we first give a short account of the general course the temperature followed in one of the boys whose case is recorded in this paper; and to render the account more intelligible, we attach a chart of his temperature variations, taken hourly for two entire days.

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* Read February 11, 1869. See Proc. Roy. Soc. vol. xvii. p. 287.

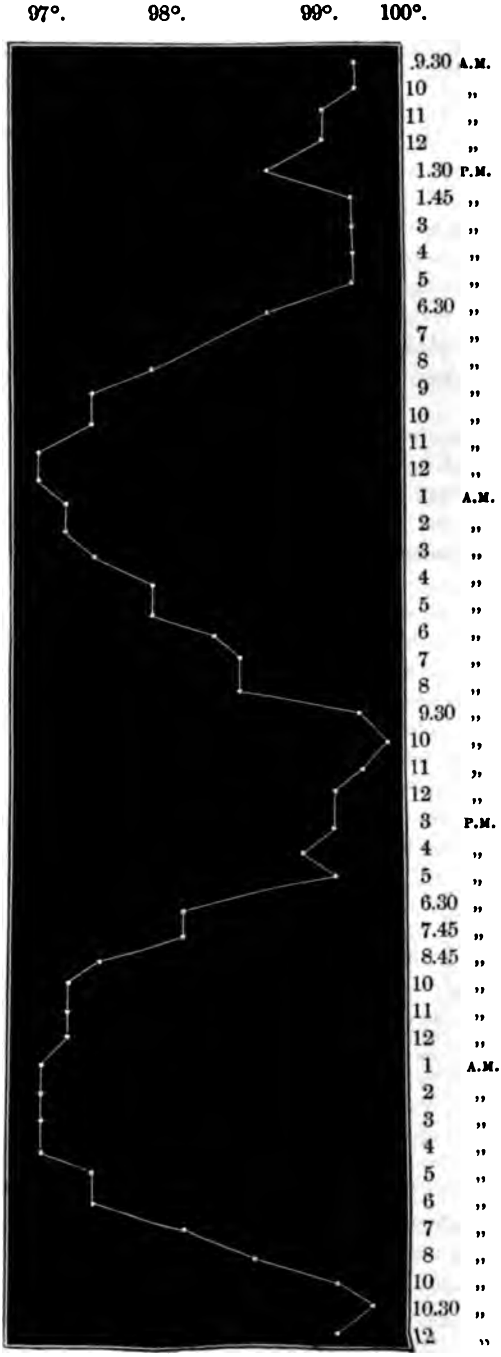

Chart of the Temperature of Alfred Mountain.

This chart indicates that the temperature does not remain at about the same degree throughout the day and night, but shows that a considerable diurnal variation occurs. The temperature reaches its highest point at about 9 A.M., and continues much the same during the chief part of the day; while in the evening it uniformly and greatly falls and remains at its lowest depression during several hours of the night; but subsequently, in the early morning hours, it again uniformly and quickly rises. This diurnal rise and fall constitutes the only great variations.

We now pass on to speak first of the observations, continued hourly without any intermission throughout the day and night, made on two lads, named respectively Alfred Mountain and Alfred Rundell. By adopting this plan we shall learn what conclusions we may safely draw from those less complete observations which were discontinued at 12 P.M.

Alfred Mountain was twelve years of age, of a robust build, well nourished and hearty. He had a patch of tinea decalvans, with which exception he was in perfect health.

The other lad, Alfred Rundell, thirteen years old, was of a rather delicate constitution, but he had a good appetite and was in his accustomed health.

To render our statement the more clear, and to enable our readers to judge for themselves of the correctness of the conclusions arrived at, we have thrown all the results of the observations into the form of Tables, but the details are recorded at the end of this section †.

In the first column of the following Table is given the name of the boy; in the second the time the observation lasted; in the third the maximum temperature of the day; in the fourth the amount of diurnal variation; in the fifth the hour when the evening fall began; and in the sixth column the hour of the commencement of the morning rise.

Name.	Time of Observation.	Maximum Daily Temperature.	Amount of Diurnal Variation.	Evening Fall begun.	Morning Rise begun.
Rundell.........	10 A.M. to 9 A.M.	99°·2	2·2	6 P.M.	3 A.M.
Mountain......	*9 „ „ 9 „	99·2	2·2	6 „	3 „
	*9 „ „ 9 „	99·4	2·4	6 „	7 „
	*9 „ „ 9 „	99·4	2·2	6 „	5 „
	*9 „ „ 8.15 P.M.	99·4	2·2	5 „	
	9 „ „ 9 A.M.	98·8	2·8	2 „	4 „
	9 „ „ 9 „	99·2	2·2	4 „	3 „
	9 „ „ 9 „	98·6	1·6	4 „	4 „

This Table shows the results of observations made hourly during both night and day, for eight days. On the four days denoted by an asterisk the temperature was taken continuously; thus the observations were taken hourly without any break, from 9.30 A.M. of January 12 to 8.15 of January 15.

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† The Charts are deposited for reference in the Archives.

The amount of daily variation of January 15 is estimated from the lowest temperature of the previous evening; but on all the other days the daily variation is estimated from the highest temperature of the day to the lowest of the following night.

From this Table we gain information on several points.

Maximum Temperature of the Day.

The average maximum temperature of lads of about 12 years of age is, according to our observations, 99° Fahr. Thus the highest temperature reached was on

	°
1 day	98·6
1 „	98·8
3 days	99·2
3 „	99·4
Average	99·1

Diurnal Variation of the Temperature.

	°
On 1 day this was	1·6 Fahr.
3 days	2·2 „
3 „	2·4 „
1 day	2·8 „

It thus appears that the diurnal variation in the temperature of boys about 12 years of age is 2°·2 Fahr.

Time when the highest Temperature of the Day is reached.

This occurred at some period between the hours of 9 A.M. and 6 P.M. Thus on

1 day the highest temperature was at	2 P.M.
1 „	from 8 A.M. to 10 A.M.
1 „	at 10 „
1 „	„ 10 „
1 „	„ 4 P.M.

The Hour of the Day when the Evening Fall begins.

It will be subsequently more fully shown that during the early morning hours the temperature quickly rises, and then during the greater part of the day remains at about its highest point. In the evening it again quickly falls, until the lowest temperature of the day is reached. The time of day when the temperature of these lads begun to fall varied very greatly; thus on

1 day the evening fall began at	2 P.M.
2 days „ „ „	4 „
3 „ „ „ „	5 „

8 days the evening fall began at	6 P.M.
3 " " " "	7 "
1 day " " "	9 "

In this Table we have not restricted ourselves to the observations continued during both day and night, but have used other observations in which the temperature was noted only till a late hour in the evening, as these, it is obvious, may be fairly used for our present purpose.

This Table proves, still more strongly than a foregoing one, that if our observations are continued till 8 P.M. our observations will include the maximum daily temperature of the day in boys of the age of these lads.

It shows also that in persons of their age the evening fall usually begins between 5 P.M. and 7 P.M.; but it likewise shows that exceptions occur, for the evening fall may begin either before or after the time stated. The fall, however, happens more frequently before than after.

The Hour when the Morning Rise begins.

This varies greatly. Thus on

4 days this began at	3 A.M.
1 day " "	4 "
1 " " "	5 "
1 " " "	6 "
1 " " "	7 "

From this we may conclude that in lads of about 12 years old the morning rise usually begins between 3 A.M. and 7 A.M.

The chief part of the rise was completed by 9 A.M. After this hour the temperature usually remained at much the same height, until the evening fall began. Thus on

4 days the temperature remained much the same from 9 A.M. to 5 P.M. = 8 hours.
1 day " " " 10 " 6 " = 8 "
1 " " " " 10 " 5 " = 7 "
1 " " " " 9 " 4 " = 7 "

On one day the temperature remained much the same from 8 A.M. to 1 P.M., then fell very slowly till 8 P.M.; afterwards the fall was much more rapid.

Hence it would appear that in boys of the age of 12 the temperature continues at about its maximum for 7 to 8 hours.

The Time at which the Maximum Depression was reached

varied greatly in these lads. Thus on

1 day it was reached at	7 P.M.
2 days " "	9 "
1 day " "	10 "
1 " " "	11 "
1 " " "	12 "
2 days " "	1 A.M.

On six of these eight days the lowest temperature of the day was reached by 12 P.M.; on two days this was not attained until 1 A.M.; but the fall in the temperature after 12 P.M. on these two occasions was but slight, in each case amounting to only 0°·4 Fahr.

It thus appears that if in other persons of about the age of these lads our observations be continued till 12 P.M., we shall obtain the whole, or nearly the whole, of the daily variation of the body temperature.

The temperature does not begin to rise immediately after its minimum has been reached, but remains at about its lowest point during several hours before the morning rise begins; thus on

2 days	it remained	at about its	minimum	2	hours.
1 day	„	„	„	4	„
1 „	„	„	„	5	„
2 days	„	„	„	8	„
1 day	„	„	„	11	„

We thus see that in these observations the maximum depression is usually of shorter duration than the maximum elevation.

Thus we have learned that observations continued from 9 A.M. to 12 P.M. give us not merely the highest temperature of the day, but likewise the hour when the temperature begins to fall—the chief, and in many cases the whole, of the evening fall. With this information we are enabled to use some other though less complete observations, in which the temperature was not observed during the night after 12 P.M. We shall thus be able to confirm or to correct by a larger number of observations the foregoing conclusions.

On the Temperature of Healthy Persons of different Ages.

In this section we give the results of observations of persons of various ages in whom the temperature was taken only till 12 P.M. On two men, however, of about forty years of age, the observations were continued throughout the night and day; and we are therefore able to compare the temperature of the whole twenty-four hours of persons of different ages.

The observations were made on the following persons:—

James Redfern, a child five years old, convalescent from rickets. His appetite was good. The ends of the bones were still enlarged, and there was some flattening and depression of the axillary regions of the chest.

Thomas Thompson and Alfred Rundell, aged respectively 11 and 13, were both in excellent health.

Frank Legg, 23 years old, convalescent from a very slight attack of rheumatic fever. He was free from pain, and his appetite was good.

Cornelius Farmolow, 40 years of age, and Alfred Purse, 55 years old, were both in good health.

Joseph Garnes, 48, had commencing locomotor ataxy. He was an unusually stout man with a capital appetite.

John Hilton, 68, suffered from slight rheumatic pains. His joints were slightly enlarged. His appetite was good.

Alfred Mountain we have already referred to.

We have, as on a previous occasion, thrown all the details of these observations into the form of a Table; the observations are given in detail at the end of this section and are deposited in the Archives.

Name.	Condition.	Date.	Time observation continued.	Max. temp.	Min. temp.	Daily variation.	Evening fall begun.
Redfern, æt. 5	Convalescent from Rickets	Dec. 15	9.25 A.M. to 12 P.M.	99°·2	96°·8	2°·4	2 P.M.
		„ 16	9.30 „ to 12 „	98·8	97	1·8	2 „
		„ 20	9.15 „ to 12 „	99	97	2	3 „
Gibbs, æt. 11.........	Convalescent from Typhoid Fever	Dec. 7	9.30 „ to 12 „	98·6	96·4	2·2	10 A.M.
		„ 8	9.30 „ to 12 „	99	97	2	7 P.M.
		„ 9	9.30 „ to 12.30 A.M.	99	97	2	7 „
Thompson, æt. 11...	Healthy......	Dec. 8	9.30 „ to 12 P.M.	99·6	97·6	2	7 „
		„ 9	12 „ to 12.30 A.M.	99·4	97·2	2·2	4 „
		„ 10	9.30 „ to 12.30 „	99·4	97·4	1·6	6 „
		„ 12	9.30 „ to 12 P.M.	99·2	97·2	2	7 „
Rundell, æt. 13......	Healthy......	Dec. 6	6 P.M. to 12 P.M.	99·2	97·2	2	8 „
		„ 17	4 „ to 1 A.M.	99·2	97·4	1·8	7 „
		„ 18	4 „ to 12 P.M.	98·8	97·2	1·6	7 „
Legg, æt. 23	Convalescent from Rheumatism	Dec. 10	9.30 A.M. to 12.30 A.M.	99	97	2	8.30 „
		„ 11	9 „ to 1 „	99·2	96·8	2·4	
		„ 12	9 „ to 12 P.M.	99·2	97·2	2	
		„ 14	4 P.M. to 12 „	98·8	97·4	1·4	
Mountain, æt. 10 ...	Healthy......	Dec. 13	11 A.M. to 12 „	98·2	...	1	9 „
		„ 14	9 „ to 12 „	99·2	...	2	6 „
		„ 15	9.30 „ to 12 „	99·2	...	1·6	6 „
		„ 16	9.20 „ to 12 „	99	...	1·6	7 „
		„ 17	9 „ to 12 „	99·2	...	1·8	7 „
		„ 28	2.45 P.M. to 11 „	99	...	1·2	7 „
		„ 29	9.15 A.M. to 12.15 A.M.	99·2	...	2·2	6 „
Farmelow, æt. 40...	Healthy......	Dec. 7	24 hours	98·6	97·2	1·4	9.30 A.M.
		„ 8	„	98·6	97·2	1·4	9.30 „
		„ 9	„	98·6	97·4	1·2	5 P.M.
		„ 10	„	98·4	97·6	0·8	3 A.M.
		„ 11	„	99	98·2	0·8	
		„ 19	„	99	98·4	0·6	
Garnes, æt. 48	Slight locomotor ataxy	Dec. 7	„	99·2	98·2	1·0	
		„ 5	„	99	...	0	
			„	99	98·9	1·0	
		„ 3	„	99·4	98·4	1·0	
		„ 4	„	99·6	98·8	0·8	
Purse, æt. 55.........	Healthy......	Dec. 7	9.30 A.M. to 12 P.M.	98·4	97·6	0·8	
Hilton, æt. 68	Chronic Rheumatism	Dec. 7	9.30 „ to 12 „	98·6	98	0·6	

We next extract from this Table the average maximum temperature and the average daily variation at different ages.

5 years of age. Redfern. 3 observations.

Average maximum temperature	99° Fahr.
Average diurnal variation	2°·06 Fahr.
Evening fall begun between	2 P.M. and 3 P.M.

10 and 11 years. Mountain, Gibbs, and Thompson. 14 observations.

Average daily maximum temperature	99°
Average daily variation	1°·8
Evening fall begun between	4 P.M. and 7 P.M.

It begun once at 10 A.M. and once at 9 P.M.

23 years. Legg. 3 observations.

Average daily maximum temperature	99°·1
Average daily variation	2°·1
Evening fall begun	6 P.M. and 8 P.M.

Average of all the foregoing observations, which were all made on persons under 25 years of age:—

Average maximum daily temperature	99°
Average daily variation	1°·9

Evening fall begins usually between 2 and 8 P.M.

These general conclusions are almost identical with those afforded by the more complete observations made on Mountain and Rundell, and we think they may be accepted as generally true of all persons under 25 years of age.

We now give the averages of the persons over 40 years of age:—

40 years. Farmelow. 6 series of observations, which were continued through the day and the night.

Average maximum daily temperature	98°·7
Average diurnal variation	1°·0

48 years. Garnes. 5 series of observations, each continued for 24 hours.

Average maximum daily temperature	99°
Average diurnal variation	0°·76

55 years. Purse. One observation.

Maximum temperature	98°·4
Daily variation	0°·8

68 years. Hilton.

Maximum temperature	98°·6
Daily variation	0°·6

Averages of all the persons over 40 years of age:—

Average maximum daily temperature	98°·8 Fahr.
Average daily variation	0°·87 „

We are now in a position to compare the temperature of persons under 25 with that of persons over 40 years of age.

Average daily maximum temperature of all the observations made on persons under 25 years	99° Fahr.
Average daily variation of the temperature of the same persons	1°·9
Average maximum temperature of persons over 40	98°·8
Average daily variation	0°·87

From this Table it appears that the daily variations in old people is considerably less than that of young people; in fact the variations in persons over 40 is only half that of persons under 25 years.

On the other hand, the daily maximum temperature is much the same both under 25 and over 40.

But the difference is not merely in the amount of depression, but in the manner of its occurrence. In young people we get in the evening a very rapid fall, and the minimum temperature of the day is quickly reached; often, indeed, in three or fours hours. In persons over 40, whose temperature was taken through the day and night, so rapid a fall rarely occurs; but the temperature usually declines very slowly, and as soon as the minimum is reached it again begins to rise, so that not only is the amount of the evening fall less in these older persons, but the period of the depression is also shorter, generally very much shorter.

It may here be noted that on some days, even when the temperature was observed for twenty-four hours, no diurnal variation occurred in persons over 40 years of age. But the temperature of these older differs in yet another respect from that of younger persons; thus apparently the diurnal fall does not observe any particular time, but occurs sometimes in the middle of the night, and at other times in the morning at about 9 A.M.

PART II.

Concerning the influence of Food on the Temperature of Healthy People.

In a previous section a considerable diurnal variation in the temperature, often amounting to two degrees, has been shown to occur.

It is important to ascertain whether this variation is produced by food or is due to other causes. Our present section is devoted to the solution of these questions.

We shall at first turn our attention to the observations made on the lads Rundell and Mountain. As these were continued night and day, without any interruption, and as we have accurate accounts of the quantity and the nature of their food, it is obvious we shall obtain much more trustworthy conclusions from these than from less elaborate and exact observations.

These two lads were about twelve years of age. The observations from which we now draw our conclusions will be found in full at the end of the sections treating of the temperature of health, and on the influence of baths on the heat of the body.

We hope to show the high probability that the diurnal variation of the temperature is altogether independent of food; and, while attempting to establish this, we shall speak first of the influence of breakfast, secondly of the tea, and lastly of the dinner.

Concerning the behaviour of the temperature before and after breakfast, it may be stated that these boys took breakfast at 6 A.M., dinner at 12, and tea at 5 P.M.—no food being allowed at other times, with one exception, which at the proper place will be noted and referred to.

Is the daily rise of the body temperature due to food? During the early morning hours, before breakfast, the temperature rose considerably; indeed rather more than half the diurnal rise took place before 6 A.M., the breakfast time of these lads, showing that some of this rise is not due to food. This part of the rise, it must be apparent, could not be produced by food, unless, indeed, it be attributed to the tea of the previous evening, a supposition impossible to maintain, as ten hours elapsed between the tea and the beginning of the morning rise. The chief influence of a meal on the body is expended during the third to the fifth hour, and then gradually declines and usually ceases altogether in ten hours, a fact shown by the variations in the urea of the urine after a meal. During the three first hours the quantity of urea gradually increases, then for an hour or more the maximum quantity is maintained, after which it gradually grows less, till the standard of inanition is reached.

Now in all our observations during the early hours of the evening, when the influence of the evening meal was greatest, the temperature fell considerably, and the rise of temperature did not begin till the effects of food must have entirely ceased.

We are thus driven to admit that half the daily rise is due to other causes than food.

The annexed Table gives a summary of the observations elsewhere recorded in detail, and includes the whole of the morning rise, which was always accomplished by 9 A.M.

The first column gives the name of the lad; the second the hours before breakfast during which the temperature rose; the third the rise before breakfast; the fourth the hourly rise; the fifth the hours of observation after breakfast; the sixth the rise during this time; the seventh the hourly rise after breakfast; and the last column gives the food taken during the investigation.

Name.	Temperature taken from	Temp. rose.	Rose per hour.	Temperature taken from	Rise.	Rise per hour.	Breakfast.
		°F.			°F.	°	
Mountain	3 A.M. to 6 A.M.	1·0	0·33	6 A.M. to 9 A.M.	0·6	0·2	Tea with little milk in it.
	3 " " 6 "	1·0	0·33	Ditto.	0·6	0·2	
	2 " " 6 "	1·2	0·3	"	0·4	0·13	Breakfast-cup of cocoa; & ½ round of bread & butter.
	2 " " 6 "	1·0	0·25	6 " " 8 "	0·8	0·4	Cup of cocoa.
	3 " " 6 "	2·0	0·66	6 " " 9 "	0·6	0·2	"
	1 " " 6 "	1·2	0·24	Ditto.	0·6	0·2	"
	3 " " 6 "	0·8	0·26	"	1	0·33	"
	2 " " 6 "	1·4	0·35	"	0·6	0·2	"
	4 " " 6 "	0·8	0·4	"	1·6	0·53	¼ pint of milk, ½ round of bread.
	2 " " 6 "	1·0	0·25	"	1	0·33	Cup of cocoa.
	4 " " 6 "	0·4	0·2	"	1·0	0·33	"
	3 " " 6 "	1·2	0·4	"	0·4	0·13	"
Rundell...	2 " " 6 "	1·2	0·3	"	0·8	0·26	½ pint of milk, ¼ round of bread and butter.
	Average............		0·328	Average............		0·264	

To what extent was the elevation following breakfast due to that meal? Probably to a very small extent; for:—

1st. The rise after breakfast was somewhat less than the rise before it, the Table showing that the average hourly rise of all the observations before breakfast was 0°·328 Fahr., while the hourly average rise after this meal was 0°·264 Fahr. It thus appears that, apart from food, the causes which influence the diurnal variation are adequate to explain the rise which happened after breakfast.

2nd. The rise after breakfast if due to food should be in proportion to the quantity taken; but no such relation occurred. The rise, as the Table shows, was somewhat greater after a cupful of very weak tepid tea than after a fairly hearty meal.

3rd. On the morning when these boys took only a cupful of tea, they may be considered to have practically gone without breakfast; yet on these days the rise took place as usual, showing that the greater part of the after breakfast rise, and in all likelihood the rise on other days, must be independent of food.

Still restricting our attention to the observations on Mountain and Rundell, we pass on to consider how far the evening fall in the temperature is affected by the evening meal.

These boys took tea at 5 P.M., always a very hearty meal, often the largest of the day, and generally consisting of two, often of three eggs, bread and butter, with tea or cocoa. They had no supper, and were allowed to fall asleep at their pleasure, and were not awakened when the temperature was read off. Due care was taken that they were well covered with bed-clothes.

In sixteen observations the temperature after tea fell immediately and continuously, until it reached the lowest point of the night. In many cases the evening fall had begun before the tea; in one case only did it rise after the meal, and on this occasion to the extent only of 0°·2 Fahr., and for a very short time.

On these occasions, as the fall occurred immediately after, and notwithstanding a hearty meal, and moreover as the boys were in all respects placed in the same conditions throughout the day, it is obvious that we must admit that there are certain circumstances irrespective of food which determine the diurnal variation of the temperature. Is all this fall due to these unknown circumstances? Or must we admit that a portion of the previous rise is due to the food, and as its heat-producing effects wear off, and as the inanition period is reached, a fall takes place equal in amount to the previous rise determined by the food?

We will now give reasons which lead us to conclude that the total of the fall is due to circumstances wholly apart from the food.

The question to decide is, how much of the evening fall is to be accounted for by those circumstances (irrespective of food) that determine the diurnal variation, and how much, if any, is due to the withdrawal of the influence of food on the body. It must be evident that the fall which occurred while the influence of the food was at its highest must be wholly attributed to other circumstances. We need hardly dwell on this point; for if the fall is due to the diminished or exhausted influence of the food, such a decline could not occur when the meal was exerting its greatest effects; therefore that part only of the fall which took place when the effects of the meal had begun to diminish or had ceased can be attributed to the withdrawal of the influence of food. But in all cases the principal part of the evening fall occurred while the influence of the tea must have been at its height, and in many instances the whole of this fall had taken place at this time—the chief part of this evening fall having on all occasions occurred before 11 P.M., and was, indeed, very often by this time at the lowest point. Thus in those cases only where the fall continued after 11 P.M., a very small portion, if any, of this fall could be due to the declining influence of food.

In this Table we give, in the first column the date, in the second the amount of fall after 10 or 11 P.M., and in the third the hour of the night when observations were discontinued.

On the following days the boy took no bath:—

Date.	Amount of fall.	Observation discontinued
Dec. 13	.... No fall after 10 P.M.	at 12 P.M.
,, 14	.... ,, 11 ,, ...	,,
,, 15	.... ,, 10.30 ,, ...	,,
,, 16	.... A fall of 0°·4 after 10 ,, ...	,,
,, 29	.... No fall after 10 ,, ...	12.15
Jan. 2	.... ,, 10.30 ,, ...	through the night.
,, 6	.... A fall of 0°·8 after 11 ,, ...	,,

Date.	Amount of fall.	Observation discontinued
Jan. 12	A fall of 0°·4 after 10 P.M.	through the night.
„ 13	„ 0°·2 „ „ „	„
„ 14	„ 0°·8 „ „ „	„

On the following days a bath was given to the boy in the morning.

Date.	Amount of fall.	Observation discontinued
Dec. 18	A fall of 0°·6 after 10 P.M. ..	at 12 P.M.
„ 20	„ 0°·2 „ „ „	„
„ 21	No fall after 10 „	„
Jan. 1	„ „ 11 „	through the night.
„ 6	A fall of 0°·2 after 10 „	„
„ 8	„ „ „ „ „	„
„ 9	„ „ „ „ „	„
„ 10	„ 0°·4 „ „ „	„
„ 11	„ 0°·2 „ 11 „	„

Summary of this Table.

Observations recorded on ten days when no bath was taken:—

In 5 of these no fall after 10.
„ 1 a fall of 0°·2 „ „
„ 2 „ 0°·4 „ „
„ 2 „ 0°·8 „ „

Observations recorded on 9 days when baths were taken in the morning:—

In 2 there was no fall after 10 to 11 P.M.
„ 4 a fall of 0°·2 occurred „ „ „
„ 1 „ 0°·4 „ „ „
„ 1 „ 0°·6 „ „ „
„ 1 „ 1°·0 „ „ „

These two Tables may, we think, be fairly thrown together, for we cannot understand how baths given in the morning should interfere with the evening temperature. Thus we have 19 series of observations; and in 7 of these no fall occurred after the chief influence of the meal had ceased.

In 5 cases a fall of 0°·2 occurred; but we think a variation so slight may be disregarded, as it might, and probably was often, caused by some accidental circumstance, as the movement of the body in bed or some similar cause—an assumption strengthened by the fact that this slight decline was often noted only once, and at the time of the next observation the temperature had recovered itself.

These observations, therefore, may be thrown together, giving the result that in 12 of the 19 observations the food appears to have had no influence on the evening fall. Of the remaining 7 observations, after the supposed influence of the food had passed away,—

In 3 there was a fall of 0°·4
„ 1 „ „ 0°·6
„ 2 „ „ 0°·8
„ 1 „ „ 1°·0

It appears, then, that the fact of the fall being in the majority of cases independent of the food, is presumptive evidence that the fall which occurred on some occasions after 11 must be due to other causes than the withdrawal of the influence of food.

The Influence of Dinner on the Temperature.

Observations made on the temperature before and after dinner furnish * further illustration of the non-influence of food on the temperature of the body. If food can raise the temperature, we should certainly expect this to occur after dinner, especially in Mountain's case, if we bear in mind that his breakfast was so small, that its effects on the body must have ceased altogether before 12, his dinner hour; and hence the influence which food can be supposed to exert on the temperature would in his case be manifest after dinner.

The appended Tables give the results of the observations made on Mountain and Rundell. We have not restricted ourselves to those days when the temperature was continuously taken through both night and day, this limitation being evidently unnecessary, but we have used all the observations made on these lads. In every case their breakfast was taken at 6 A.M., and their dinner at 12, or shortly after this.

Name and Date.	Time before dinner.	Rise or Fall.	Hourly.	Dinner	Time after dinner.	Rise or Fall.	Quantity of food taken at dinner.
Mountain.							
Dec. 14	9 to 12	Rise 0°·6	0°·2	12	12 to 3	Rise 0°·2	Full dinner.
					3 " 5	Fall 0·4	
" 15	10 " 12	" 0·6	0·3	12	12 " 3	Rise 0·2	"
					3 " 5	Fall 0·2	
" 16	9 " 12	" 0	0	12	12 " 3	Fall 0·2	"
					3 " 5	Rise 0·4	
" 29	9 " 12	" 0·4	0·13	12	12 " 3	Rise 0·4	"
					3 " 6	Fall 0·8	
" 30	9 " 3	" 0·4	0·06	3	3 " 5	0·0	Dinner moderate.
Jan. 2	9 " 12	" 0	0	12	12 " 3	Rise 0·6	"
					3 " 4	Fall 0·4	
" 6	9 " 12	Fall 0·6	0·2	12	12 " 1	Rise 0·4	Full dinner.
					1 " 5	Fall 0·4	
" 12	9 " 12	" 0·2	0·06	12	12 " 3	Rise 0·2	"
					3 " 5	0·0	
" 13	9 " 12	At first a rise of 0°·2 & then a fall of 0°·4	0·06	12	12 " 3	0·0	Slight dinner.
					3 " 5	0·0	
" 14	10 " 12	0	0·0	...	12 " 3	Fall 0·2	Full dinner.
					3 " 5	Rise 0·6	
Rundell.							
Jan. 6	10 " 1	0	0·0	1	1 " 4	Rise 0·2	"
					4 " 6	Fall 0·2	

---

* The detailed observations will be found in the Charts which give the diurnal variation of the temperature and the influence of bath (Royal Society's Archives).

This Table shows the results of eleven observations; and we gather that the rise between 9 A.M. and 12 (noon) was always very small; and on some days none took place, as the hourly rise during these 3 hours on

4 occasions was	*nil*
1 occasion it was	0°·2
1 " "	0°·3
1 " "	0°·13
2 occasions it was	0°·06

In two instances there was an hourly fall, once of 0°·2 and once of 0°·6.

We now add a Table indicating the alterations in the temperature during three hours after dinner.

	°
On 4 days the temperature rose	0·2
" 2 " it fell	0·2
" 2 " it rose	0·4
" 1 day it rose	0·6

On two days there was no alteration.

So slight a rise of 0°·2 is so easily produced by accidental causes, that, as we have before said, it may be almost disregarded. On this supposition, then, it appears that on six of the ten days the rise was *nil* or practically unappreciable, and on other days the rise was very small.

It may be fairly said that we shall elicit surer information as to the influence of the dinner, if we observe how the temperature comported itself between three and five o'clock, when the food influence on the body may be supposed to be at the maximum. The following Table exhibits the temperature during these hours, in the case of Mountain:—

On 2 days the temperature remained unaltered.

	°
" 2 " it fell	0·2
" 3 " "	0·4
" 1 day "	0·8

The temperature rose on two days only, and on each instance 0°·4.

Here we close our investigations on the influence of food on the lads Mountain and Rundell. We cannot absolutely say that food does not in any case raise the body temperature; yet it is clear that its influence in this respect, if any, on persons in health and well nourished is very slight indeed.

We may further add that these observations are opposed to the statement made by Dr. John Davy, to the effect that a fall in the temperature occurs immediately after food.

We give a few further tabular observations on the after-dinner temperature made on Redfern, Gibbs, Thompson, Farmelow, and Garnes.

Name.	Date.	Dinner hour.	Rise in temperature after dinner.
Redfern	Dec. 15	12	0°·0
	" 21	"	0·4
Gibbs	" 7	"	Constant fall.
	" 9	"	0·0
Thompson...............	" 8	"	0·0
Farmelow	" 9	"	0·6
	" 10	"	0·0
	" 11	"	0·4
	" 19	"	0·2
Garnes	" 5	"	0·0
	" 6	"	0·0
	" 7	"	0·6
			Average rise 0°·18

Thus in seven of these twelve observations no appreciable rise took place.

In five the temperature rose, the average rise being 0°·4. Breakfast was taken at 6 A.M., and no further food allowed afterwards until 12. It may be objected, in respect of these observations, that the influence of the breakfast had not passed away by dinner time, and that the temperature elevated by this meal was maintained by the dinner. As the influence of the breakfast may not have passed off by dinner time these observations are not so trustworthy in the settlement of this question of the influence of food on the temperature as those made on Mountain. We give them, however, for what they are worth, believing they serve in some degree to corroborate our previous conclusions.

It is, however, right to refer to some observations in which a very considerable rise occurred after a late breafast.

These were made on three persons in ill-health. One man, 35 years of age, suffered from hemiplegia; another, 32 years, with hemiplegia and dilated heart; and the third, 21 years old, was the victim of some obscure disease. His spleen was very much enlarged, and he occasionally passed large quantities of blood with his urine. These patients abstained from food from tea (5 P.M.) one day till breakfast on the following day, taken either at 10 A.M. or a little later.

These observations, for the sake of clearness, are thrown into a Table. The first column shows the time before breakfast the temperature was taken; the second gives the temperature of the body at the time of commencing each observation; the third shows the amount of the rise before breakfast; the fourth gives the rise after breakfast; and the fifth column shows the duration of the rise.

Time before breakfast.	Temperature.	Rise before food.	Rise after food.	Duration of the Rise.	
h m	°	°	°	h m	
1 15	96·6	0	1·2	45	Remained fixed.
50	97·4	0·4	1·0	40	Remained the same.
1 5	98	0	0·6	1 15	" "
45	97	0	1·4	50	Then fell 0°·4.
3 15	97·6	0	0·4	4 15	
1 5	97·6	0	1·0	1 25	
45	97·6	0	0·2	45	
1 10	96·6	0	0·8	1 5	
3 15	...	0·4	0·8	2 15	
Average	...	0·09	0·82	1 28	
45	98	0·2	0·8	18	Mary Lyon.

It thus appears that after breakfast the temperature of these men rose considerably, the average being 0°·8, sufficient, it may be said, to show that the food is capable of causing an elevation in the temperature of the body, especially as in many of the days no rise took place before breakfast, the temperature having been taken at this time for several hours.

We are constrained to admit that perhaps this rise in the axilla was due to the food; although opposed to this admission, we might urge the fact that on three days, when the temperature of Jones and Tarves was taken during the morning hours, while they were kept fasting, an average rise of 0°·7 occurred, being almost identical with that which took place on other days when they partook of food.

Assuming that this rise in the axilla was really due to food, How was the rise caused? Was it due to a general elevation of the body temperature, owing to increased consumption of the food? or was it due to other causes? It could hardly be due to increased oxidation of food, for we should expect the elevation to be most marked at the time during which the food was most rapidly absorbed and carried to the tissues. But in these men the rise in the temperature occurred much sooner than this, being completed on an average in an hour and twenty-eight minutes, and in several cases in fifty minutes, in some even in thirty, although there was a considerable rise on these days; yet at the time the influence of food would be expected to be most marked the temperature either remained unchanged or in some instances it even fell.

Further, it may be remarked that were the rise after breakfast due to combustion of the food, we should expect that a similar rise would occur after the dinner, which was a heartier meal than the breakfast; but the rise after dinner was much less than that after breakfast.

TARVES.				
Nov. 13.	Dinner at 1.	No rise.	Observations continued till	5 P.M.
„ 15.	„ 4.20.	Rise 0°·6	„	8 „
JONES.				
Nov. 9.	„ 4.10.	„ 0°·6	„	8 „
„ 10.	„ 1.15.	„ 0·4	„	5 „
„ 11.	„ 1.10.	„ 0·8	„	5 „
„ 14.	„ 12.15.	„ 0·4	„	6 „
„ 13.	„ 1.30.	„ 0·4	„	5 „
PERRIN.	„ 4.10.	No rise.	,	8 „
MARY LYON.	„ 1.30.	Rise 0·2	„	5 „
	Average rise	0·37		

It may, however, be fairly objected to this argument, that the influence of the breakfast on the body had not ceased before that of the dinner begun, and that the dinner prevented the possible fall after the breakfast if dinner had not been taken.

Thus, to summarize very generally the conclusions we have arrived at:—There is a diurnal cycle in the temperature of the body, much more evident in young than in middle-aged or old people; in elderly people, indeed, the daily variation is often very slight, and sometimes altogether absent; further, this diurnal variation is due to other causes than food, which if operative in any degree in producing elevation of the temperature, is so, if at all, to a very small extent.

PART III.

On the Influence of Cold Baths on the Temperature of the Body.

We undertook these experiments from a wish to learn the influence of the cold bath on the temperature of the surface and internal parts of the body, and to ascertain if it were possible, in any degree, to cool the whole body; and if so, to what extent the temperature could be lowered, and how long this depression would continue.

Our observations were made in the winter months. The temperature of the person fixed upon for experiment was first carefully ascertained either in the axilla, the rectum, or under the tongue. He was then immersed in a bath of a temperature of 60° Fahr. for a time varying from 1 to 35 minutes in the several series of experiments. The body was covered by the water up to the chin. Immediately on the return to bed the temperature was again ascertained, and every few minutes afterwards. In some instances we took the temperature under the tongue during the immersion; but these notes we fear are not quite correct, although we spared no pains, for the patient's teeth chattered so much as to prevent the steady closure of the lips.

We shall now give a tabular extract of the effects of the cold bath, first on the heat of the surface, and afterwards on that of the internal parts.

Name.	Time of bath.	Temp. fell in axilla to	Mainly recovered in	Entirely recovered in	Temp. fell in mouth or rectum.	Recovered in
	mins.	°	mins.	mins.		
Redfern, æt. 5	1	95·5	25	50		
	1	95	13	48		
	5	93·4	67	77		
	5	93	48 ?	80		
	5	92	58	123		
	5	92	75	120		
	5	92	70	75		
	15	90	88	98		
	30	87	105	125		
	35	89	135	135		
Gibbs, æt. 11.........	1	96	45			
	1	97	48		°	mins.
Thompson, æt. 11...	1	97	33	60	0	0
	5	95	20	100	0·6	100
Mountain	10	94·6	50	50	1·8	
Legg, æt. 23	10	96	22			
Mountain, æt. 10 ...	16	...	...	...	1·2	69
	30	...	...	...	2·8	43
	1	...	...	...	0·8	70
	5	...	...	...	0	
	15	...	...	...	1·1	
Walker, æt. 48	15	92	...	280	1·1	280
Purse, æt. 55.........	5	93·6	...	...	2·2	2 hours 17 mins.&more
Hilton, æt. 68	5	95·3	37			

This Table exemplifies, as was to be expected, that the cold baths considerably cooled the surface of the body, and that the longer the immersion the greater was the depression of the temperature of the skin. Thus this Table shows that on some occasions the temperature of the axilla fell to 87° Fahr.; but a depression so great occurred only when the bath was continued for a considerable time, for a period, indeed, varying from 15 to 35 minutes; and even this amount of depression was of short duration. To illustrate this effect we give three extracts from the preceding Table. A bath of 60° Fahr. for one minute reduced the temperature of a child five years old to 95° Fahr., but in 13 to 25 minutes the temperature of the axilla had nearly recovered itself. With a bath of the same temperature continued five minutes we reduced the temperature to 92°; but in 48 to 75 minutes after leaving the bath the axillary temperature had nearly recovered. Nay, even after a bath of 35 minutes the temperature only fell to 89°, and recovered itself in 135 minutes. The greater part of the lost heat was very quickly restored, and then the axilla more slowly regained the rest of the lost temperature.

Influence of Cold Baths on the Temperature of internal parts of the Body.

Cold baths reduce the temperature of internal parts of the body, but the reduction is considerably less than that of the surface. Thus we lowered the temperature under the tongue or in the rectum from 0°·6 to 2°·8 Fahr., the amount of depression being, of course, proportional to the duration of the bath.

The duration of the depression varied greatly in different persons. Thus on some occasions the lost heat was restored in an hour; in others not for several hours, three, four, or more; but very soon after leaving the bath the greater part of the lost heat was restored, the remaining slight depression passing off much more slowly. Our observations elicited the rather curious fact that the time the body takes to recover its temperature holds no relation to the amount of depression; thus in some instances, when with great depression, the recovery was speedy, while on other occasions, with but slightly depressed temperature, the recovery was slowly effected.

We noticed, too, that many times the internal parts did not become cooled under the actual operation of the bath, but the temperature of the internal parts fell gradually for some time after the bath. The explanation we would offer of this curious fact is, that during the bath much heat is withdrawn from the surface, and the cold contracting the blood-vessels lessens greatly the quantity of blood passing through the skin. Heat is thus not abstracted from deep parts by cooling of the blood, but simply by conduction. On quitting the bath, the blood-vessels of the skin become widely dilated, and the blood then passing quickly through the cold skin becomes cooled, and reduces the temperature of deep parts.

Influence of the Cold Bath on the Temperature of the Body after the discontinuance of the Bath.

We first give the hour when the evening fall begun on the bath days:—

Redfern	12 to 4 P.M.
Gibbs	3 P.M.
Thompson	4 to 6 P.M.
Mountain	6 P.M.

On comparing this Table with the one given in the section treating of the temperature in health, it will be found that the time when the temperature begun to fall is much the same whether a bath be given or not.

For the sake of comparison we put side by side the hour the evening fall begun on bath and non-bath days.

Name.	No bath.	Bath.
Redfern	2 to 3	2 to 4
Thompson and Mountain	4 to 7	4 to 6[*].

[*] It hitherto escaped our notice that the younger the person the sooner in the day does the evening fall begin. Thus in Redfern it began between 2 and 3, in Thompson and Mountain between 4 and 7, and in Purse and Hilton between 9 and 11.

Amount of evening fall on bath days:—

Name.	Amount of fall.	Obs. continued till
Redfern	2°·2	12
Thompson and Mountain..	2·1	12

We extract from a previous section the amount of the evening fall on non-bath days:—

Redfern................	2°·06	12
Mountain	2·2	whole night
	1·8	12

These Tables show that the bath exerts no influence on the evening fall.

Table of the hour when the minimum temperature of the day is reached on bath and non-bath days, showing that, in this respect, cold baths have little or no influence:—

Bath days.	
Redfern................	between 5 and 8.
Mountain and Thompson..	between 10 and 12.
Non-bath days.	
Redfern................	6.30 to 8.
Mountain and Thompson..	9 to 12.

PART IV.

Influence of Hot-water and Vapour Baths on the Temperature of the Body.

We next turn our attention to the influence of hot-water and vapour baths on the temperature of the body; and we shall first attempt to ascertain the influence of hot baths on the temperature of the body, both at the time of immersion and during the rest of the day. We have, as usual, extracted and tabulated the chief data from the charts appended to the end of this section. The temperature was obtained by placing the thermometer under the tongue while the person was in the bath. The method of taking the temperature is always noted in the charts.

Name.	Date.	Temp. of bath.	Time in bath.	Rise of temp. of the body.	Evening fall of temp. begun.	Amount of evening fall.	Observations continued to	Min. temp. reached.
			mins.	°F.	P.M.		P.M.	P.M.
Mountain, æt. 10 ...	Dec. 21	106°	20	3·4	12	1°·8	12	10
	Jan. 1	105 to 108°	24	4·6	...	2·0	All night.	11
	„ 3	97 to 101	134	1·8	5 & 8	2·6	„	11
	„ 4	99 to 103	40	2·6	8	2·2	„	11
	„ 5	99 to 101	45	1·8	6	2·2	„	12
							P.M.	
Thompson	Dec. 6	100 to 103	20	1·0	5	2·0	12	11
Rundell	„ 20	105	25	1·6	6	1·4	12	9
	„ 21	106 & 107	15	2·8	7	1·4	12	10
Ryman, æt. 25	„ 19	105	20	3·4	9	2·2	12	11
	„ 20	105	20	4·6	...	1·6	12	
	„ 21	Hot	20	1·8	...	1·2	12	
	„ 22	105	20	2·8	...	1·8	12	
Tarves, æt. 32	...	107 to 109	40	4·6				
Luff, æt. 16	Jan. 22	106	87	3·6				

From this Table we learn that in all cases the temperature of the body rose in the hot bath, this elevation, as might be expected, varying with the temperature and duration of the bath. It varied from 1° Fahr. to 4°·6 Fahr.; or, to put it in another and more striking way, we raised the body temperature to 103° and 104° Fahr., a severe fever height. Is it possible to bring the temperature of the body to that of the bath? As it is impossible to remain in a very hot bath sufficiently long to enable us to determine this question, we must examine those experiments in which the temperature of the bath was not very high. We were compelled in a short time to discontinue the experiment with a very hot bath on account of the great weakness induced by it. Yet as the temperature continued to rise so long as the patient remained in the hot water, it is probable that, if adequate time could have been allowed, the temperature of the body would have become identical with that of the bath.

The charts show that a bath of moderate temperature, as 101° to 102°, will raise the body temperature to that of the bath. Whilst making several of these series of observations the bath temperature was recorded simultaneously with the temperature of the body, and the figures are given in the charts, as with Mountain on Jan. 3rd, 4th, and 5th, and with Mooney and Luff on Jan. 22nd. Thus it is clear that the temperature of the body may be raised to that of a bath at 101°·8.

Influence of Hot-water Bath on the Temperature of the Body during the rest of the day after the Bath.

The evening fall begun in Mountain, Thompson, and Rundell between 5 P.M. and 8 P.M.

The average diurnal variation of all the observations in the above-named lads, all about the same age, was..	1°·9 Fahr.
Of Mountain alone	2°·1 ,,

The minimum temperature of the day of these three lads was reached between 9 and 11 P.M., the average time being 10.

If these results are compared with those obtained from the observations made on the same lads on the bathless days, it will be seen that the two sets of figures entirely agree; that, in fact, hot-water baths, except at the time of immersion, exert no influence on the body temperature.

On the Influence of Hot-vapour Baths on the Temperature of the Body.

The temperature of the body is always raised by the hot-vapour bath, the amount of elevation, as might be expected, being proportionate to the heat and duration of the bath, as shown in the following Table:—

Breathed the steam of the bath.

Name.	Temp. of body before bath.	Raised during bath to	Amount of elevation.	In	Temp. of bath.
	°	°	°	mins.	°
Mooney..	98	101·4	3	25	103
„	..	100·2	..	..	101?
„	98·8	96·6	0·8	33	100
„	98·4	100	1·6	64	97° and 98°
Church ..	98·8	100·4	1·6	28	90° to 94°
		Breathed air of outer room.			
Luff	..	101	..	..	103° to 105°

In some of the observations we have stated only the time occupied in effecting the rise, as on these days the bath, before it was entered, was first heated to the temperature at which it was maintained; while on the other days the bath was entered immediately the steam was turned on, being thus occupied while the temperature of the room was rising.

These experiments enable us to learn the rapidity with which the body may lose heat; for as the body of the person experimented upon was heated considerably higher than could be maintained by physiological processes, so this unnatural elevation is speedily lost by evaporation, radiation, and conduction. As the loss of heat sustained varied with the temperature to which the body was raised, we shall speak first of the cooling of the body from a high temperature, and next from a temperature a little above that of health. It was found that heat was much more quickly lost under the first than under the latter conditions.

We now give a Table to show the rapid loss of heat when the temperature of the body has been raised considerably, for instance to 102° and 104° Fahr.:—

Name.	Date.	Fall.	in
		°F.	
Mountain......	Dec. 21	2·2	10 minutes.
Rundell	Dec. 21	1·8	10 „
Ryman........	Dec. 18	3·0	13 „
„	Dec. 22	2·2	8 „
Mooney	Jan. 18	1·4	10 „

A degree of heat was thus lost in 4·7 minutes. As an interval of five minutes sometimes elapsed between the testings, we can hardly speak with the exactness indicated by the figures. It is better to say the body may lose a degree of heat in less than five minutes.

The following Table shows that the loss of body heat is much less rapid when the body is less heated, one degree being lost in 40 minutes:—

After hot-water bath.

Name.	Date.	Fall from	of	in
	Jan. 18	100°	0°·6	37 minutes.
Mountain ..	Dec. 21	100	0·6	13 "
" ..	Jan. 1	100	0·6	20 "
Rundell	Dec. 21	100	0·8	20 "
Thompson ..	Dec. 6	100	0·6	25 "
Ryman	Dec. 21	100·2	1·0	57 "

After hot-vapour bath.

Name.	Date.	Fall from	of	in
Mooney		99·4	0·6	33 minutes.
"		100	0·6	36 "
"		100·4	1·2	42 "
Luff..................		100	0·6	15 "
Church		100·4	0·6	20 "

Can the figures last given be accepted as about the usual rate at which heat is lost by the body in health?

We shall now try to ascertain the source of this increase of body heat under a warm-water or hot-vapour bath.

Our experiments with vapour-baths afford the more complete solution; and the following remarks will be limited to them, though it is evident that they will also hold good in the case of hot-water baths.

This increase in the heat of the body may be due to

1. Accumulation of heat in the body.
2. Absorption of heat from the bath.
3. Both of these circumstances.

Some of the increased heat was undoubtedly due to an accumulation of heat in the body.

It is evident that a person in a hot-vapour bath of the temperature of the body, and when breathing the steam, could lose no heat by evaporation, or radiation, or conduction, the only means of withdrawing heat from the body. If the production of heat is not diminished *pari passu* with its loss from the body, then heat will accumulate, and the temperature of the body will be raised.

Two observations show this to be the case. Thus with Mooney on Jan. 29th and Church on Jan. 22nd the temperature of each respectively was raised to 100° and 100°·4, while the bath was never more than the temperature of their bodies. We show this in a Table:—

Name.	Temp. of body before bath.	While in bath.	Rise.	Temp. of bath.
Mooney ..	98°·4	35 mins.	100° = 1·6	97° and 98°
Church ..	98·8	24 "	100·2 = 1·4	highest 94

Here it was impossible that the heat which raised the temperature of the body could be obtained from the bath, itself never hotter than the bodies of the boys before undergoing the experiment. If the formation of heat was lessened so as exactly to meet the diminished loss by conduction and radiation, the heat of the body would have continued at the same point; but this was not the case, and thus it appears that the formation of heat cannot be suddenly lessened to such an amount.

Thus the increased heat of the body ensuing from a vapour-bath is certainly in part due to accumulation of heat, which under other circumstances is lost by evaporation and radiation. Is all the heat imparted to the body by the vapour-bath to be accounted for in this way? Some part must, in the nature of things, be absorbed from the bath, and is evidenced in our experiments, the elevation being too rapid to be caused solely by an accumulation of heat in the body.

May 3, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Lord Coleridge and the Right Hon. Sir Henry Bartle Edward Frere, K.C.B., whose certificates had been suspended as required by the Statutes, were balloted for and elected Fellows of the Society.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Prof. James Dewar, M.A.
Sir Joseph Fayrer, M.D., K.C.S.I.
Rev. Norman Macleod Ferrers, M.A.
Thomas Richard Fraser, M.D.
Brian Haughton Hodgson, F.L.S.
John W. Judd, F.G.S.
William Carmichael M'Intosh, M.D.
Robert M'Lachlan, F.L.S.
Prof. John William Mallet, Ph.D.
Henry B. Medlicott, M.A.
Henry Nottidge Moseley, M.A.
Prof. Osborne Reynolds, M.A.
William Roberts, M.D.
Prof. James Thomson, LL.D.
Prof. William Turner, M.B.

The following Papers were read:—

I. "Further Observations on the Modification of the Excitability of Motor Nerves produced by Injury." By GEORGE J. ROMANES, M.A., F.L.S. Communicated by Prof. BURDON SANDERSON, M.D., LL.D., F.R.S. Received April 11, 1877.

In my former paper on this subject* I showed, among other things, that injury of a motor nerve is attended with a very marked and peculiar alteration in its relative excitability towards the stimuli which are respectively supplied by closing and opening a voltaic circuit. In the present paper I propose to detail the results which have been obtained by continuing this line of research; and, in order to render them more easily intelligible, I shall begin by briefly restating such of the previous results as form the basis of the present ones.

It will be remembered, then, that my method of experimenting was as follows. Having pithed a frog and laid it on a frog-board in such a position that one of the hind legs should hang over the edge of the board, I divided the tendo Achillis, dissected out the gastrocnemius as far as its point of origin, and removed the tibia just below the knee. The exposed though uninjured gastrocnemius was then laid with its flat surface on non-polarizable electrodes, in such a way that while one electrode supported the extreme tarsal end of the muscle, the other supported its extreme femoral end. By means of a rheochord it was then ascertained what strength of voltaic stimulus the muscle required to give its earliest response, (*a*) to the anodic† make, (*b*) to the kathodic make, (*c*) to the anodic break, and (*d*) to the kathodic break. It will be remembered that, under these circumstances, "the muscle is usually more sensitive to minimal stimulation supplied by *closure* of the constant current when the femoral end rests on the kathode [case (*b*)], than when this end rests on the anode [case (*a*)]. Conversely, under similar circumstances, the gastrocnemius is more sensitive to minimal stimulation supplied by *opening* of the constant current when the femoral end rests on the anode [case (*c*)], than when this end rests on the kathode [case (*d*)]. In view of the other facts of electrotonus the present ones are of interest, because, as the sciatic nerve enters the gastrocnemius near the femoral end of the latter and then spreads out its peripheral ramifications as it advances, in the experiments just mentioned one electrode is in almost immediate contact with the nerve-trunk where it enters the muscle, while the other electrode supports the part of the muscle that contains only peripheral nervous elements. It is therefore to be expected, upon the theory of electrotonus, that the muscle under these conditions should prove itself most sensitive to the closing excitation when the nerve-trunk rests on the kathode, and most sensitive to the opening excitation when the nerve-trunk rests on the anode. . . . .

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* Proc. Roy. Soc., May 4, 1876.

† Throughout this paper I shall designate the direction of the voltaic current through the gastrocnemius by employing the terms "anodic" and "kathodic" with reference to the *femoral* end of the muscle, *i. e.* where the nerve-trunk first enters the latter. Thus, for instance, "anodic make" means closure of the current in a direction descending from the femoral to the tarsal end of the gastrocnemius.

"If the gastrocnemius of a frog be placed on non-polarizable electrodes in the position just described, and if care has been taken not to injure the attached sciatic nerve, I find that upon now dividing this nerve, either near or just within the muscle, remarkable alterations ensue, not only, as is already known, in the *general* sensitiveness of the muscle, but also, and more particularly, in its *relative* sensitiveness to make and to break of the current. . . . For just as before cutting the *normal* sensitiveness of the muscle is greatest to the closing excitation when its femoral end (or uninjured nerve-trunk) rests on the kathode, and most sensitive to the opening excitation when this end rests on the anode, so, after the general sensitiveness has been exalted by cutting, the *exaltation* shows itself in a far higher degree to the closing excitation when the femoral end (or severed nerve-trunk) rests on the kathode, and to the opening excitation when this end rests on the anode."

Having thus described the qualitative effects of nerve-injury in relation to electrotonus, my former paper went on to describe also the quantitative effects; but for my present purpose it is unnecessary to quote the latter. For having observed that the particular effects of nerve-injury which I was investigating decreased with great rapidity after the first infliction of the injury, I deemed it desirable to confirm the quantitative results already published by employing a more rapid method of varying the intensity of the voltaic current. Accordingly, instead of using the rheochord, I introduced into the exciting circuit a rheostat consisting of a long U-tube charged with dilute solution of zinc sulphate. Into each leg of the U-tube there dipped a zinc rod of the same length as the tube. These two rods formed part of the circuit, and as, by means of an appropriate mechanical arrangement which need not be described, they could be slid up and down the legs of the tube with great facility, the resistance offered by the tube could thus be varied with great rapidity.

In some other respects, also, I changed the method. Instead of non-polarizable electrodes I used platinum plates measuring 4 millims. across. Also, in order to estimate the *maximum* effect produced by nerve-injury in each of the four cases (*a*), (*b*), (*c*), and (*d*), I only made one comparative observation on every muscle I employed. That is to say, if I wished to ascertain the *maximum* degree in which the excitability of a nerve is increased by section in any one of these four cases, I began by observing, in the uninjured nerve, the *maximum* number of Ohm's units of resistance which I could afford to throw into the U-tube, so as only just to procure a response to the make or break stimulus as the case might be. Having noted this, I raised the sliding rods to the top of the U-tube, so as to throw in the entire resistance of which the tube was capable, *i. e.* a greater resistance than could possibly be required to cause minimal stimulation in the next stage of the experiment. I now cut the gastrocnemius through at its extreme femoral end; and the same instant that I did so I began rapidly to pass the sliding rods down the U-tube with one hand, while with the other hand I closed and opened the current a number of times in as rapid succession as possible. Having observed the point at which the responsive contraction was first given, and throwing away that particular muscle, I repeated the experiment with another muscle, and so on —never using the same muscle for more than one such observation, and so always obtaining a record of the *maximum* increase of excitability *immediately* after infliction of the injury.

The results of a number of experiments conducted on this improved method confirm, in the main, those previously obtained. As before, however, I encountered immense individual variations in different muscles, and therefore, as before, I here select mean cases for quotation. It is only necessary further to explain that in the appended Table the figures represent the number of Ohm's units of resistance which, with two Grove's cells, I required to introduce in each of the eight cases before I procured minimal stimulation.

	Anodic make.	Kathodic make.	Anodic break.	Kathodic break.
Before cutting	90,000	100,000	14,000	6,000
After cutting......	140,000	300,000	195,000	14,000

These proportions, as already observed, agree pretty closely with those which I obtained by the method previously employed. Such differences as exist are to be explained, partly by the superiority of the later method, and partly by the fact that in the one series of experiments I employed *Rana temporaria*, while in the other series I employed *R. esculenta*—the muscles of the latter species being less excitable than those of the former. It is interesting to note that the chief difference in the two series of results has reference to the kathodic make, and that the difference is of such a kind as to render the degree in which the excitability is increased by section in this case more proportional to the degree in which it is increased in the case of the anodic break. The two cases, however, are still very far from being *numerically* proportional, the degree of increase in the two cases being respectively represented by the numbers 1 : 3 and 1 : 14 (nearly). To explain this numerical discrepancy, therefore, we must still resort to the considerations set forth in my previous paper (see vol. xxv. pp. 12, 13). I may here add that in some instances of *maximum* increase of excitability, due to nerve-injury, I have observed the kathodic make to rise from 80,000 to 600,000 Ohms, and the anodic break from 15,000 to 400,000 Ohms.

§ 2. The rapidity with which this abnormal excitability declines after the injury is, as already stated, considerable. The following instances, which refer to the anodic break, will serve to show this:—

Time.	Degree of Excitability in Ohms.
Before cutting	13,000
2 seconds after cutting	280,000
30 „ „	244,000
1 minute „	210,000
2 minutes „	170,000
3 „ „	150,000
4 „ „	134,000
5 „ „	100,000

Another instance:—

Before cutting	22,000
2 seconds after cutting	300,000
30 „ „	230,000
1 minute „	180,000
2 minutes „	150,000
3 „ „	130,000
4 „ „	110,000
5 „ „	95,000
6 „ „	80,000
7 „ „	73,000
8 „ „	67,000
9 „ „	60,000
10 „ „	54,000
15 „ „	25,000
20 „ „	13,000

I may here state that if the excised gastrocnemius be inserted under the skin of a freshly killed frog, and the latter be kept in a moist cool place, the nerve will sometimes retain its irritability for 48 hours or more—the muscle, when placed on the electrodes at the end of that time, still continuing to respond to the kathodic make and to the anodic break. But of course a very much stronger current is now required to produce these responses than was required to do so when the nerve and muscle were in a fresh state.

§ 3. A strong voltaic current, or a strong induction-shock, allowed to break into an uninjured nerve-trunk, causes in the latter an increase of excitability analogous to that which is caused by mechanical injury. Thus, for example, a momentary exposure of an uninjured sciatic to the full strength of a single Grove's cell caused the excitability of the nerve towards the breaking excitation, supplied through the same electrodes by a small Daniell's cell, to rise from 5000 to 100,000 Ohms. Similarly a strong induction-shock supplied by a single Grove's cell with the secondary coil at zero, and thrown in between the electrodes from a small Daniell's cell, caused the excitability of the nerve towards a closing stimulus supplied by the latter to rise from 40,000 to 185,000 Ohms. In conducting these experiments, I was not able to perceive that the *direction* of the strong or injuring current made any difference in the nature of the results.

§ 4. This concludes my observations so far as stimuli of minimal intensity are concerned; and, at the suggestion of Dr. Burdon-Sanderson, I terminated this inquiry regarding the electrotonic condition of injured nerves by substituting for voltaic stimuli of minimal intensity, voltaic stimuli of minimal duration. The method which I employed in this part of the research was as follows:—The frog (*R. temporaria*) having been prepared as already described in § 1, the duration of the voltaic stimulus was graduated by means of a heavy pendulum, which constituted one pole of the battery, and which, while swinging, made contact at the lowest point in its arc with the other pole. The latter consisted of a fixed platinum wire placed vertically, and the contact was made with it by means of a pointed piece of metal attached to the moving pole and placed horizontally. Thus by increasing or diminishing the distance through which the pendulum, or moving pole, was allowed to swing, a stimulus of any required duration could be supplied to the muscle interposed in the circuit. As a battery I employed a single Daniell's cell; and, lastly, I interposed a rheochord, a commutator, and a key. Such being the apparatus, the course of any one experiment was very simple. By means of the swinging pendulum, the uncut muscle was supplied with a stimulus of measured duration, which was then graduated down to the point at which the break of the current succeeded the make with a rapidity just sufficiently great to prevent the muscle from responding to either stimulus, (*a*) when the femoral end rested on the anode, and (*b*) when this end rested on the kathode. These two durations having been noted, the nerve was cut through at the usual place, and the observations (*a*) and (*b*) repeated as rapidly as possible. It was invariably found that in both cases a much shorter duration of the voltaic stimulus was required to produce minimal stimulation than had been required to do so before the nerve was cut, the *intensity* of the voltaic current, of course, being kept uniform throughout.

An apparent objection to this method of experimenting is apt to suggest itself, viz. that the make and the break must follow one another much too rapidly to admit of the observer being able to eliminate the effects of the former from those of the latter stimulus. But, as a matter of fact, the desired elimination is performed by the nervo-muscular tissue itself. For it usually happens that a gastrocnemius presents some perceptible difference in the character of its contraction, according as the latter is given in response to make or to break of the current. Therefore, by first ascertaining, with an ordinary key, the optical appearance which the responses to make and break respectively present, it is not difficult afterwards to recognize which of these appearances is presented by the response to rapidly succeeding make and break stimuli, and so to determine which of these rapidly succeeding stimuli is the one to which the response is given. Now I found in this way that, by making the duration of contact sufficiently brief, the nerve, whether or not injured and in whichever direction the current was allowed to pass, only responded to the closing stimulus. Therefore, by choosing a strength of current which, in each of the cases (*a*) and (*b*) before nerve-injury, was just sufficiently strong to elicit a response when the voltaic stimulus was of t duration in the one case and t' duration in the other, I was sure that in each of the two cases the response which I obtained was given to the closing, and not to the opening, excitation. Having noted the values of t and t', I divided the sciatic just where it enters the gastrocnemius, and then shortened the duration of contact down to the point at which, in each of the two cases (*a*) and (*b*), the muscle again only just responded to the stimulus. Let these durations be respectively represented by T and T'. As before, I ascertained that the responses had exclusive reference to the closing excitation; so that, by recording the values of t, t', T, T', I was able to obtain for responses to stimuli of minimal duration representative numbers, such as those in the former Table, which have reference to stimuli of minimal intensity. It is only necessary further to state that, as different gastrocnemius muscles exhibit considerable variations in the degree of their natural irritability towards voltaic stimuli of short duration, and as for my purposes it was desirable to obtain a physiological, as distinguished from a physical, basis whereon to institute my comparisons, in the case of each muscle I began by graduating the *intensity* of the current down to that point at which the duration of contact required to produce minimal stimulation before nerve-injury was the same as it had been in my previous experiments. Or, in other words, by appropriately varying the intensity of the current to suit the degree of excitability manifested by each particular muscle before injury, I was able, notwithstanding the differences in excitability presented by different muscles, to render t a constant. When this was done, however, t', T, and T' were all found more or less variable in different muscles—as, of course, we should expect from the analogous case of responses to stimuli of minimal intensity. I therefore tabulated the results yielded by twenty gastrocnemius muscles, and then calculated the average duration of contact which in each of the cases (*a*) and (*b*) before cutting, and (*a*) and (*b*) after cutting, was required to cause minimal stimulation. It is the averages so obtained which are rendered in the following tabular statement of results:—

	Anodic make.	Kathodic make.
Before cutting	·00533 sec.	·00439 sec.
After cutting............	·00311 sec.	·00117 sec.

Concerning these results it is only necessary to observe that there is a tolerably close parallel between them and those which were previously obtained by employing stimuli of minimal intensity. That is to say, in the case of the anodic make the increase of excitability due to injury is, roughly estimated, in the proportion of about 3 : 5, and *this whether such increase is estimated by employing stimuli of graduated intensity, or stimuli of graduated duration*; and similarly in the case of kathodic make, though the proportions here yielded by the two methods are not quite so equal as in the other case. But this general parallelism between the quantitative results yielded by the two methods in the case of both the closing excitations serves but to render more conspicuous the difference in the results yielded by the two methods in the case of the anodic opening excitation; for while in the first of the two methods, viz. that in which stimuli of minimal intensity were employed, it was found that after injury the excitability of a nerve towards the anodic-break stimulus is greater than it is towards the anodic-make stimulus, such is not found to be the case when, as in the second of the two methods, these two stimuli are made to follow one another in very rapid succession. I can only explain this fact by supposing that, for the breaking excitation to be fully effectual, a certain interval of time is required for the nerve to become polarized by the passage of the voltaic current; and, therefore, that, in my experiments with currents of minimal duration, a response to the anodic make was always given when a shorter duration of the current was employed than that which was required to produce a response to the anodic break[*].

§ 4. In conclusion, I may state that the period of latent stimulation does not appear to be perceptibly affected by nerve-injury.

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[*] *How much* shorter I was not able to ascertain, from the fact that the contraction due to the make and that due to the break, when they both occur together, become so blended that the eye is not able to analyze them with sufficient precision to decide at what point the anodic-break contraction first asserts itself. Hence we must rest satisfied with the general statement, that the minimal anodic-break stimulus is in some unknown degree of longer duration than the minimal anodic-make stimulus; and this even after the susceptibility of the nerve to the former stimulus is so enormously augmented by injury as, from the other method of experimentation, we know it to be.

II. "On the Temperature-correction and Induction-coefficients of Magnets." By G. M. Whipple, B.Sc., Superintendent of Kew Observatory. Communicated by Robert H. Scott, F.R.S. Received April 12, 1877.

It has been the practice at the Kew Observatory since 1856 to determine, for the use of persons about to make observations upon terrestrial magnetism, the various constants of the magnets and instruments with which they intend to observe; and a considerable number of these constants are now recorded in the books of the Observatory.

Having been frequently applied to for information respecting the constants of magnets and the method of determining them, I have extracted from our registers the values of the two most important constants, viz. that of the variation of the power of the magnet under changes of temperature, and that showing the effect of the inductive action of the earth upon the magnets, as found by experiment. The results are given in the following paper.

The magnets experimented upon may be classed under six heads (approximate dimensions):—

a. Solid cylinders, 3·6 inches long, 0·3 inch diameter.
b. Hollow cylinders (collimators), 3·65 inches long, 0·28 inch internal diameter.
c. Parallelopipeds (bars), 5·4 inches long, 0·8 inch wide, 0·12 inch thick.
d. Rhomboidal plates (dip-needles), 5·7 inches long, 0·05 inch thick.
e. Thin cylinders (enclosed in brass tubes, deflectors), 3·75 inches long, 0·30 inch diameter.
f. Various.

All of the magnets were made of the best steel, and rendered as hard as possible before magnetization, which operation has been in every case carried to saturation.

The method employed in determining the temperature-coefficient is as follows:—The magnet being firmly fixed in a water-tight wooden box, provided with a thermometer, is placed upon a frame in such a manner that its axis shall lie in the same horizontal plane with the needle of a unifilar magnetometer, at a short distance away, and adjusted until the axes of the two needles are approximately at right angles to each other, the position of the deflecting magnet being that which is designated by the Astronomer Royal as end on. Warm water at 85° F. is then poured into the box, and as soon as the magnet has become heated to this temperature the unifilar magnet is brought to rest and its position accurately read off. The water at 85° F. is then removed, and the apparatus being cooled down to 60° F., water of that temperature is placed in the box; another observation of deflection is then made, and finally the experiment is repeated with water at 35° F. After this another series of observations is commenced with water at 85° F., 60° F., and 35° F., and this is again repeated once or twice.

The observations are first corrected for changes of the earth's magnetic force during the experiment by means of simultaneous readings of the curves given by the self-recording magnetometers of the Observatory; and the reduction is performed according to the method given by the following formula, due to Prof. B. Stewart, the coefficients determined being q and q', which represent the decrease of the magnetic moment of the magnet produced by an increase of temperature of 1° F.

Demonstration of the Formula for finding Temperature-correction.

Let $\frac{m}{\mathrm{X}}=\frac{1}{2}r^3 \sin u$ be the normal equation of equilibrium at temp. t_0, then $m\{1-q(t-t_0)-q'(t-t_0)^2\}=\frac{1}{2}\mathrm{X}r^3 \sin(u-du)$ is the altered equation.

Hence $$\tfrac{1}{2}r^3\mathrm{X}\sin u\{1-\&\mathrm{c.}\}=\tfrac{1}{2}\mathrm{X}r^3\sin(u-du);$$

$$\therefore \sin u-\sin u q(t-t_0)-\sin u q'(t-t_0)^2=\sin(u-du).$$

Let $\sin u(q)=x$, $\sin u(q')=y$; then

$$x(t-t_0)+y(t-t_0)^2=\sin u-\sin(u-du).$$

Demonstration of the method of using the Self-recording Magnetographs in eliminating the effects of disturbance in ascertaining Temperature-correction.

(1) Let the Horizontal Force alter;

let $$\frac{m}{\mathrm{X}}=\tfrac{1}{2}r^3\sin u \text{ be the normal equation,}$$

and $$\frac{m}{\mathrm{X}+\Delta\mathrm{X}}=\tfrac{1}{2}r^3\sin(u-du) \text{ the altered one.}$$

Hence $$\frac{m}{\mathrm{X}}\frac{\Delta\mathrm{X}}{\mathrm{X}}=\tfrac{1}{2}r^3\cos u\,du;$$

hence $$du=\frac{\Delta\mathrm{X}}{\mathrm{X}}\tan u.$$

(2) Let the Declination alter; the equation is still $\frac{m}{\mathrm{X}}=\frac{1}{2}r^3\sin u$; hence the angle of deflection remains the same, or the deflected magnet makes the same angular change as the declination magnetograph.

Of the temperature-coefficients of 109 magnets which have been examined, the value of q' has only been determined for 79. Taking the whole series we find the following:—

	Mean value.	Average difference from mean.	Maximum.	Minimum.
q	0·000161	±0·000060	0·000762	0·000044
q'	0·00000048	±0·00000023	0·00000398	0·00000001
Subdividing them into classes we have, for				
Class A. Solid cylinders. 20 magnets examined.				
q	0·000178	±0·000048	0·000368	0·000104
q'	0·00000057	±0·00000027	0·00000129	0·00000011
Class B. Collimators. 46 magnets examined.				
q	0·000161	±0·000043	0·000399	0·000093
q'	0·00000043	±0·00000016	0·00000082	0·00000004
Class C. Bars. 7 magnets examined.				
q	0·000171	±0·000068	0·000286	0·000103
q'	0·00000064			
Class D. Dip-needles. 11 magnets examined.				
q	0·000073	±0·000027	0·000132	0·000044
q'	0·00000023	±0·00000018	0·00000054	0·00000001
Class D. (Between temperatures 10° and 60° F.)				
q	0·000095	±0·000038	0·000164	0·000054

Class E. Deflectors.				
q	0·000197	±0·000044	0·000254	0·000111
Class E. Deflectors (between temperatures 10° and 60° F.).				
q	0·000161	±0·000054	0·000323	0·000091
Class F. Various magnets.				
q	0·000418		0·000762	0·000193
q'	0·00000157		0·00000398	0·00000020

Induction-coefficients.

The induction-coefficient is determined from observations of deflection made after the well-known method of Lamont, the magnet being alternately placed with its north pole upwards and downwards, but at the same distance from a suspended needle, the difference in the amount of deflection of the latter in the two positions determining the effect of the earth's inductive action upon the magnet.

The formula, differing slightly from Lamont's, employed in the reduction, and of which the following is a demonstration, is due to the late Mr. John Welsh, F.R.S.:—

(1) It is assumed that the induction produced by the inducing action of the earth's magnetic force is distributed in the same manner throughout the bar experimented upon as its permanent magnetism.

Let μ=the increase of the moment of the magnetic bar produced by the action of an inducing force = unity.
M=the magnetic moment of the permanent magnetism of the bar.
X=horizontal component of the earth's force.
Y=vertical component of the earth's force.
ϕ=angle of deflection, north end of bar downwards.
ϕ'=angle of deflection, north end of bar upwards.
u=angle of deflection when the bar is used as an ordinary deflector at 1 foot distance.
i=magnetic dip.

(2) If c be a constant, and m the magnetic moment of the suspended needle, $c\text{M}m$ may be taken to represent the attraction of the bar (disre-

garding induced magnetism) upon the suspended needle; then, as the line joining the centres of the two magnets is in every part of the observation approximately at right angles to the suspended needle, it follows from (1) that the attraction of the bar will be proportional to its magnetism. Hence in the north end downwards the equation of equilibrium will be

$$c(\mathrm{M}+\mathrm{Y}\mu)=\mathrm{X}\sin\phi,$$

and with the north end upwards

$$c(\mathrm{M}-\mathrm{Y}\mu)=\mathrm{X}\sin\phi'.$$

Hence

$$2c\mathrm{Y}\mu=\mathrm{X}(\sin\phi-\sin\phi'),$$

$$2c\mathrm{M}=\mathrm{X}(\sin\phi+\sin\phi').$$

Hence

$$\frac{\mathrm{Y}}{\mathrm{M}}\mu=\frac{\sin\phi-\sin\phi'}{\sin\phi+\sin\phi'}=\frac{\tan\frac{1}{2}(\phi-\phi')}{\tan\frac{1}{2}(\phi+\phi')};$$

and since $\mathrm{Y}=\mathrm{X}\tan i$,

$$\mu=\frac{\mathrm{M}}{\tan i\,\mathrm{X}}\frac{\tan\frac{1}{2}(\phi-\phi')}{\tan\frac{1}{2}(\phi+\phi')}=\frac{\sin u\tan\frac{1}{2}(\phi-\phi')}{2\tan i\tan\frac{1}{2}(\phi+\phi')}.$$

From the values of observations made at the Observatory with 66 magnets, all belonging to Classes A and B, we find—

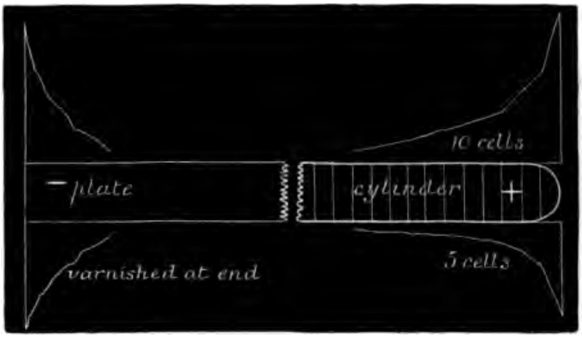
	Mean value.	Average difference from mean.	Maximum.	Minimum.
μ	0·000207	±0·000035	0·000420	0·000080

III. "Distribution of the Radicals of Electrolytes upon an Insulated Metallic Conductor." By ALFRED TRIBE, Lecturer on Chemistry in Dulwich College. Communicated by Dr. GLADSTONE, F.R.S. Received April 19, 1877.

Among other facts demonstrated in my communication to the Royal Society in January 1876 (Proc. Roy. Soc. vol. xxiv. p. 308), it was shown that a rigid conductor, when placed lengthwise between the electrodes in a fluid in the act of electrolysis, becomes, with sufficient battery-power, endowed with the power of doing chemical work similar in kind to the battery-electrodes themselves. This phenomenon, it was contended, was explicable on the view which regards an electrolyte as a dielectric, with the additional function of being capable of mutually exchanging its constituents in the act of depolarization—a conception which induced the quantitative experiments detailed below.

Distribution of the Positive Radical on the end of an insulated strip of metal facing the Positive Electrode.

For these experiments a 5-per-cent. solution of copper sulphate was employed, and a glass trough 30 centimetres long by 13 centimetres broad and 12 centimetres deep. In each experiment a strip of silver plate about the thickness of writing-paper was placed lengthwise midway between the electrodes, and allowed to remain immersed in the electrolyzing fluid for one hour, the battery-power being 5 cells of Grove. In experiments 1 and 1*a* the strips were similar, being 1 decimetre long and 1 centimetre wide. In experiment 2 the strips were of the same width, but 4 millims. longer, 2 millims. at each end being covered with varnish, with the object of eliminating the action of the end surfaces. Successive lengths of 2 millims. exactly were cut off after the action, and the copper thereon was determined by the cyanide of potassium and ammonia method. One centimetre of the cyanide solution was equivalent to ·00309 gram of copper. The results were as under:—



Lengths of 2 millims. commencing at the end facing the + electrode.	Cubic centimetres of KCy used.		
	1.	1*a*.	2.
1.	7·2	7·3	5·4
2.	5·0	4·4	4·2
3.	4·0	3·6	3·6
4.	3·1	3·1	2·8
5.	2·9	2·4	2·4
6.	2·1	2·3	2·3
7.	1·9	1·8	1·4
8.	1·6	1·4	1·3
9.	1·1	1·2	1·0
10.	0·6	0·5	0·6
11.	trace.	0·0	0·0

It is apparent from the numbers that there is a considerable accumulation of the positive radical (and hence it may be concluded of negative electricity) at the extremity of the strip in proximity to the positive electrode, and that this gradually diminishes until, at a distance of 22 millims. from the end, it becomes too small for estimation. The relatively small number in the first line of experiment 2 shows the influence of the end surface.

The subjoined Table exhibits the results, showing the distribution upon a diamond-shaped strip (1 decimetre in its longer and 1 centimetre in its shorter diagonal) placed lengthwise under similar conditions:—

Lengths of 2 millims. counting from positive electrode.	Cubic centimetres of KCy used.	
	3.	3a.
1.	2·6	3·1
2.	2·0	1·7
3.	1·4	1·8
4.	1·6	1·7
5.	2·1	1·8
6.	1·9	2·1
7.	2·0	1·4
8.	1·7	1·6
9.	1·3	1·3
10.	1·0	1·2
11.	1·0	0·5
12.	0·4	0·0

It is worthy of notice that the positive radical is detectable at a somewhat greater distance from the point of the rhombus than from the end of the rectangular strips.

The area of the first 2 millims. of the rhombus equals 0·4 square millim. Calculating the copper on the first 2 millims. of the rectangular strip for an equal area, the accumulation on these areas is shown to be greater on the rhombus in the ratio of 20 to 1. This illustrates to a considerable extent the power of the more pointed conductor in storing up the radical.

Distribution of the Negative Radical on the end of the conductor facing the Negative Electrode.

For this a cylinder of pure copper, 1 decimetre long and 1 centimetre in diameter, was placed lengthwise in the trough containing a 5-per-cent. solution of copper sulphate, its position being similar to that of the silver strip in the previous experiments. The end of the cylinder, which was so placed as to be eaten away in the experiment, was made up of sections of 3 millims. each screwed together. The first cylinder had six and the other two twelve divisions each. The amount of action was found by weighing the sections before and after subjecting the cylinder to the action of the electrolyzing fluid.

In experiment 4, three cells of Grove were used, the time being two hours; in experiment 5, five cells for one hour; and in 6, ten cells for one hour.

No. of section, counting from the end opposite the negative pole.	Weight in grams of copper dissolved.		
	4.	5.	6.
1.	·0923	·0936	·1590
2.	·0532	·0428	·0982
3.	·0368	·0325	·0745
4.	·0343	·0277	·0568
5.	·0285	·0226	·0507
6.	·0241	·0184	·0464
7.	......	·0164	·0391
8.	......	·0138	·0341
9.	......	·0120	·0271
10.	......	·0100	·0280
11.	......	·0084	·0202
12.	......	·0062	·0168

In experiment 4, signs of corrosion were noticeable to 48 millims. from the end of the cylinder, while the deposit of copper reached 46 millims. from the other end. In experiment 6, corrosion was noticeable also to 48 millims. from the one end, and deposition to 46 millims. from the other end. From this it would appear that the electro-negative radical spreads over a greater surface than does the positive radical, or that the neutral line is not in the middle of the cylinder, but somewhat removed towards the negative end, that is, where the deposition of the positive radical takes place. I hope to return to this.

The numbers in the columns 1, 1*a*, and 2 may be taken as showing approximately the distribution of the positive radical or negative electricity on the respective silver strips, and the numbers in 5 and 6 that of the negative radical or positive electricity on the copper cylinders, which facts, graphically represented, give figures as in the diagram.

The Society adjourned over Ascension Day, to Thursday, May 17.

May 17, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Hyperjacobian Surfaces and Curves." By WILLIAM SPOTTISWOODE, M.A., Treas.R.S. Received April 23, 1877.

(Abstract.)

In a paper published in the 'Mathematische Annalen' (vol. iii. p. 459) Brill has discussed the question of curves having three-point contact with a doubly infinite pencil of curves, and in particular he has investigated some of the properties of the curve passing through all the points of contact with the individual curves of the pencil. In the same journal (vol. x. p. 221) Krey, of Kiel, has applied a method similar to that of Brill with partial success to the question of curves having four-point contact with a triply infinite pencil. Some formulæ, however, given in my paper "On the Sextactic Points of a Plane Curve" (Phil. Trans. 1865, p. 657) have proved to be directly applicable to both questions. An application of them to Brill's problem will be found in the 'Comptes Rendus' for 1876 (2nd semestre, p. 627), and a solution of Krey's problem in the 'Proceedings of the London Mathematical Society' for the same year (vol. viii. p. 29).

The present subject was, in the first instance, suggested by the foregoing papers; and from one point of view it may be regarded as an attempt to extend the question to the case of surfaces; viz. to determine a curve which shall pass through the points of contact of a given surface U with certain surfaces belonging to a pencil V, and to investigate some of its properties.

From a slightly different point of view, however, it may be considered as an extension of two ideas, viz:—first, that of the jacobian surface, or locus of the points whose polar planes with regard to four surfaces meet in a point; and secondly, that of the jacobian curve, or locus of points whose polar planes with regard to three surfaces have a right line in common. More particularly, commencing with the facts, first, that if a surface of the form $a\phi+b\psi+c\chi$ touch a surface U, the point of contact is a point on the jacobian, and, secondly, that if a surface of the form $a\phi+b\psi$ touch a surface U, the point of contact is a point on the jacobian curve, I have endeavoured to extend them to higher degrees of contact.

The properties here considered are those which appertain to the points, if any, through which all the surfaces pass, or, as they may be termed, the *principal points* of the system, and they consist mainly in the nature of the contact of the hyperjacobian surfaces with the surface U, and the multiplicity of the hyperjacobian curve at the points in question.

The present investigation extends to the cases of two-branch contact of the given surface with a one-fold and with a two-fold pencil, and of three-branch contact with a four-fold pencil. In the latter case notice is also made of some properties appertaining to the points, if any, where all the surfaces touch one another, or, as they may be termed, the *secondary points* of the system. In particular it is shown that in the case of common, or two-branch, contact and a one-fold pencil the jacobian curve has a double point at the principal points; while in the case of three-branch contact and a four-fold pencil the hyperjacobian curve has a triple point at the same points.

II. "On the Length of the Spark from a Voltaic Battery in different Gases at ordinary Atmospheric Pressures." By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. Received May 17, 1877.

We venture, in anticipation of a more detailed account, in course of preparation, of our experiments with the chloride-of-silver battery which have engaged our attention for more than three years, to lay before the Society the results of some investigations we were induced to make in consequence of phenomena we have observed in the voltaic discharge in different residual gases contained in vacuum tubes. We have found, as we anticipated we should do, that the length of the spark at ordinary atmospheric pressures in the following gases is the longest in the order in which they are enumerated—hydrogen, nitrogen, air, oxygen, carbonic acid—it being nearly twice as long in hydrogen as in air. The spark in air between a point (positive) and a plate (negative) with our battery of 8040 cells is about 0·34 in., and in hydrogen 0·60. We may mention that we are making up our battery to 10,440 cells. The length of the spark does not appear to be dependent on the specific gravity of the gas, but may have some relation to its viscosity.

III. "Further Researches on the Deportment and Vital Resistance of Putrefactive and Infective Organisms, from a Physical point of view." By JOHN TYNDALL, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution.

(Abstract.)

For reasons which will appear in the sequel, it will be desirable to glance, in the first place, at the results already submitted to the Royal Society.

Portions of the autumn of 1875, and of the winter and spring of 1875–76, were devoted to the first section of these researches, and on the 13th of January, 1876, its main results were communicated orally to the Royal Society. The completed memoir was handed in to the Society on the 6th of April: it is published in vol. 166 of the 'Philosophical Transactions.'

Many of the 'closed chambers' employed in the inquiry were submitted on the 13th of January to the inspection of the Fellows. There had been over fifty of them in all, and several of them had been used more than once. The air in these chambers had been permitted to free itself from floating matter by self-subsidence, no artificial means of cleansing it being employed. Sterilized organic liquids and infusions of the most varied kinds freely exposed to air thus spontaneously purified were found, when tested by the microscope, to remain absolutely free from organisms of all kinds, and equally free from the turbidity, scum, and mould which to the naked eye are the infallible signs of the generation and multiplication of such organisms.

These experiments embraced, among others, the following organic liquids :—urine in its natural condition; infusions of mutton, beef, pork, hay, turnip, sole, haddock, codfish, salmon, turbot, mullet, herring, eel, oyster, whiting, liver, kidney, hare, rabbit, barndoor fowl, pheasant, and grouse.

The number of separate vessels containing these liquids which were exposed to spontaneously purified air amounted to several hundreds, and the consensus of their testimony, in the sense just indicated, was complete.

Five minutes' boiling was found in all cases sufficient to sterilize the infusions.

When, after remaining sterile for months, the doors of the chambers were opened so as to admit the uncleansed air of the laboratory, the contact of such air, or, more correctly, of the matter mechanically floating in it, infallibly produced organisms in abundance—sometimes exclusively Bacterial, sometimes exclusively fungoid, and sometimes a combination of both.

Infusions of the substances above referred to were afterwards exposed in succession to air which had been freed of its floating matter by filtration through cotton wool, also to air from which the floating matter had been removed by calcination, and finally to vacua obtained by exhausting as far as possible with an air-pump large receivers which had been previously filled with filtered air.

Boiled for five minutes and exposed to air thus treated, or to vacua thus produced, none of the infusions showed subsequently any alteration of colour or transparency to the naked eye, or to the microscope any manifestation of life.

Thus far I have summed up the results obtained with self-purified air, filtered air, calcined air, and air-pump vacua, the liquids in all cases being exposed in open test-tubes. Small retort-flasks were afterwards resorted to. Charged with the infusions, they were boiled in heated oil or brine, and sealed with exceeding care during ebullition. At the Royal Society on January 13th, 1876, one hundred and thirty such flasks were submitted to the Fellows, free alike from putrefaction and from life. They embraced specimens of all the substances above mentioned and some others.

Briefly expressed, then, the evidence furnished by six months' assiduous work during the autumn, winter, and spring of 1875–76, proved conclusively that in the atmospheric conditions then existing in the laboratory of the Royal Institution, not one of the many hundred flasks and tubes experimented on failed to be sterilized by five minutes' boiling, and no countenance was given to the notion that any of these once sterilized infusions possessed the power of spontaneously generating life.

The investigation embodied in the memoir now submitted to the Society was opened in the summer of 1876 by a series of tentative experiments on turnip infusions, to which were added varying quantities of bruised or pounded cheese. Seven different kinds of cheese were employed, fifty-seven test-tubes being charged with the mixture and exposed to the self-purified air of closed chambers.

The majority of these mixtures remained unchanged; a minority became charged with organisms, which are, in my opinion, completely accounted for by reference to the protective action of the cheese. In the memoir of which this is an abstract such protective action is illustrated by the fact that when ordinary mustard seeds were tied together in a calico bag, they resisted the boiling temperature for a considerable multiple of the time which sufficed to kill them when no bag enveloped them. The bag and outside seeds protected the interior ones.

Not temperature alone, but the ability to diffuse its juices or salts, appears to be a condition of importance in the destruction of the integrity and life of a germ by boiling water. Without diffusion a germ may withstand temperatures competent to destroy it where diffusion is free.

I need not remark on the imperviousness of cheese to water, and its consequent power to prevent diffusion.

These summer experiments on turnip-cheese infusions were, however, merely tentative, and I purpose completing them hereafter.

In the autumn I resumed experiments on infusions of hay, which had been purposely postponed. With this substance no difficulty was encountered in my first inquiry. Boiled for five minutes, and exposed to air purified spontaneously, or freed from its floating matter by calcination or filtration, hay infusion, though employed in multiplied experiments at various times, never showed the least competence to kindle into life. After months of transparency, I have, in a great number of cases, inoculated this infusion with the smallest specks of animal and vegetable liquids containing *Bacteria*, and observed, twenty-four hours afterwards, its colour lightened and its mass rendered opaque by the multiplication of these organisms.

But in the autumn of 1876, the substance with which I had experimented so easily and successfully a year previously appeared to have changed its nature. The infusions extracted from it bore, in some cases, not only five minutes' but fifteen minutes' boiling with impunity. On changing the hay a different result was often obtained. Many of the infusions extracted from samples of hay purchased in the autumn of 1876 behaved exactly like those extracted from the hay of 1875, being completely sterilized by five minutes' boiling.

The possible influence of age and dryness soon suggested itself, and I tested the surmise to the uttermost. Numerous and laborious experiments were executed with hay derived from different localities; and by this means, in the earlier days of the inquiry, it was revealed that the infusions which manifested this previously unobserved resistance to sterilization were, one and all, extracted from old hay, while the readily sterilized infusions were extracted from new hay, the germs adhering to which had not been subjected to long-continued desiccation.

As the inquiry proceeded the distinction between old and new hay became more and more blurred, while prolonged experiment with hay of various kinds failed to rescue the question from uncertainty. I therefore turned to substances of a succulent nature—to fungi, cucumber, melon, beetroot, and artichoke, for example, whose parasitic or epiphytic germs were unlikely to have suffered desiccation.

Boiled for periods varying from five to fifteen minutes and exposed afterwards to moteless air, in numberless experiments these infusions broke down, charging themselves throughout with organisms, and loading themselves, almost in all cases, with a soapy corrugated scum.

I then fell back upon infusions whose deportment had been previously familiar to me, and in the sterilization of which I had never experienced any difficulty. Fish, flesh, and vegetables were resubjected to trial.

Though the precautions taken to avoid contamination were far more stringent than those observed in my first inquiry, and though the interval of boiling was sometimes tripled in duration, these infusions, in almost every instance, broke down. Spontaneously purified air, filtered air, and calcined air (calcined, I may add, with far greater severity than was found necessary a year previously) failed, in almost all cases, to protect the infusions from putrefaction.

I was sometimes cheered by a success which, at the time of its occurrence, would seem to be the result of increased severity in the methods of experiment. But the success was subsequently so opposed by failure that it finally stood out rather as an accident than as the normal result of the inquiry.

I had the most implicit confidence in the correctness of my earlier experiments; indeed incorrectness would have led to consequences exactly opposite to those arrived at. Errors of manipulation would have filled my tubes and flasks with organisms, instead of leaving them transparent and void of life. By the unsuccessful experiments above referred to a clear issue was therefore raised:—Either infusions of fish, flesh, and vegetables had become endowed in 1876 with an inherent generative energy which they did not possess in 1875, or some new contagium external to the infusions, and of a far more obstinate character than that of 1875, had been brought to bear upon them at the later date. The scientific mind will not halt in its decision between these two alternatives.

For my own part the gradual but irresistible interaction of thought and experiment made it in the first instance probable, and at last certain, that the atmosphere in which I worked had become so virulently infective as to render utterly impotent precautions against contamination and modes of sterilization which had been found uniformly successful in a less contaminated air. I therefore removed from the laboratory, first to the top, and afterwards to the basement of the Royal Institution, but found that even here, in a multitude of cases, failure was predominant, if not uniform. This hard discipline of defeat was needed to render me acquainted with all the possibilities of infection involved in the construction of my chambers and the treatment of my infusions.

I finally resolved to break away from the Royal Institution, and to seek at a distance from it a less infective atmosphere. In Kew Gardens, thanks to our President, the requisite conditions were found. I chose for exposure in the Jodrell laboratory the special infusions which had proved most intractable in the laboratory of the Royal Institution. The result was that liquids which in Albemarle Street resisted two hundred minutes' boiling, becoming afterwards crowded with organisms, were utterly sterilized by five minutes' boiling at Kew.

A second clear issue is thus placed before the Royal Society:—Either the infusions had lost in Kew Gardens an inherent generative energy which they possessed in our laboratory, or the remarkable instances of life development, after long-continued boiling, observed in the laboratory are to be referred to the contagium of its vessels or of its air.

With a view to making nearer home experiments similar to those executed at Kew, I had a shed erected on the roof of the Royal Institution. In this shed infusions were prepared and introduced into new chambers of burnished tin, which had never been permitted to enter our laboratory. After their introduction the liquids were boiled for five minutes in an oil-bath.

The first experiment in this shed resulted in complete failure. Not one of the infusions exposed to the moteless air of the shed escaped putrefaction.

Either of two causes, or both of them combined, might, from my point of view, have produced this result. First, a flue from the laboratory was in free communication with the atmosphere not far from the shed; secondly, and this was the real cause of the infection, my assistants in preparing the infusions had freely passed between the laboratory to the shed. They had thus incautiously carried the contagium by a mode of transfer known to every physician.

The infected shed was disinfected; the infusions were again prepared, and care was taken, by the use of proper clothes, to avoid the former causes of contamination. The result was similar to that obtained at Kew, viz. organic liquids which in the laboratory withstood two hundred minutes' boiling, were rendered permanently barren by five minutes' boiling in the shed.

A third clear issue is thus placed before us, which I should hardly think of formulating before the Royal Society, were it not for the incredible confusion which apparently besets this subject in the public mind. A rod thirty feet in length would stretch from the infusions in the shed to the same infusions in the laboratory. At one end of this rod the infusions were sterilized by five minutes' boiling, at the other end they withstood two hundred minutes' boiling. As before, the choice rests between two inferences:—Either we infer that at one end of the rod animal and vegetable infusions possess a generative power which at the other end they do not possess, or we are driven to the conclusion that at the one end of the rod we have infected and at the other end disinfected air.

The second inference is that which will be accepted by the scientific mind. To what, then, is the inferred difference at the two ends of the rod to be ascribed? In one obvious particular the laboratory this year differed from that in which my first experiments were made. On its floor were various bundles of old and desiccated hay, from which, when stirred, clouds of fine dust ascended into the atmosphere. This dust proved to be both fruitful and, in the highest degree, resistant. Prior to the introduction of the hay which produced the dust, no difficulty as regards sterilization had ever been experienced; subsequent to its introduction my difficulties and defeats began.

I have twice glanced at periods of boiling amounting to two hundred minutes; for, after long and laborious trials of shorter periods I advanced to longer ones, subjecting turnip and cucumber infusions to the boiling temperature for intervals varying from five minutes to three hundred and sixty minutes. Up to a certain point these liquids maintained their power of developing life, but beyond this point complete sterility was the result. In the preliminary experiments bearing upon this question the point of sterilization lay between 180 and 240 minutes. Boiled for the former period the infusions continued fruitful; boiled for the latter period they remained permanently barren.

In these and numerous other experiments a method was followed which had been substantially employed by Spallanzani and Needham, and more recently by Wyman and Roberts, the method having been greatly refined by the philosopher last named. The flasks were partially filled with the infusions, the portions unoccupied by the liquids being taken up with ordinary unfiltered air. Now as regards the death-point of contagia we know that in air it may be much higher than in water, the selfsame temperature being fatal in the latter and sensibly harmless in the former; hence my doubt whether, in my recent experiments, the resistance of the contagium did not arise from the fact of its being surrounded, not by water but by air.

I changed the method, and made a long series of experiments with filtered air. They were almost as unsuccessful as those made with ordinary air. From time to time I succeeded in producing complete sterility by five minutes' boiling; but these successes were so checked by failures that, similar to other cases referred to, they appeared in the light of accidents. They were, however, by no means uninstructive, for they revealed the existence of breaks in the prevalence of the contagium, which, under the circumstances, might have been foreseen.

A rapid glance at the means employed to improve the method of experiment, and at the results of their employment, may be permitted here. Bulbs, exhausted by an air-pump and afterwards heated almost to redness, were filled when cool with filtered air. While being charged with the infusions the bulbs were warmed, so as to produce a gentle outflow of air, and their necks were sealed while the outflow continued. It was thus sought to avoid the contamination consequent on an indraught.

The failures resulting from this mode of experiment greatly predominated over the successes.

Employing similar bulbs, their necks in the first instance were drawn out at the ends to tubes of capillary fineness. The bulbs were then filled with one third of an atmosphere of filtered air, and, while connected with the air-pump, were heated almost to redness. The capillary tubes were then sealed with the lamp; the sealed ends were afterwards broken off in the body of the liquid, two thirds of each bulb being thus filled with the infusion. By great care it was found possible to reseal the capillary tubes without removing them from the liquid. The infusions were afterwards boiled from five to fifteen minutes.

Here also the fruitfulness of the boiled infusion was the rule, and its barrenness the exception.

One source of discomfort clung persistently to my mind throughout these experiments. I was by no means certain that the observed development of life was not due to germs entangled in the film of liquid adherent to the necks and higher interior surfaces of the bulbs. This film might have evaporated, and its germs, surrounded by air and vapour, instead of by water, might on this account have been able to withstand an ordeal to which they would have succumbed if submerged.

A plan was therefore resorted to by which the infusions were driven by atmospheric pressure through lateral channels issuing from the centres of the bulbs. As before, each bulb was filled with one third of an atmosphere of filtered air, and afterwards heated nearly to redness. When fully charged, the infusion rose higher than the central orifice, and no portion of the internal surface was wetted save that against which the liquid permanently rested. The lateral channel was then closed with a lamp without an instant's contact being permitted to occur between any part of the infusion and the external air. It was thus rendered absolutely certain that the contagia exposed subsequently to the action of heat were to be sought, neither in the superjacent air nor on the interior surfaces of the flasks, but in the body of the infusions themselves.

By this method I tested in the first place the substance which, at an early stage of the inquiry, had excited my suspicion—without reference to which the discrepancy between the behaviour of infusions examined in the winter of 1875–76 and those examined in the winter of 1876–77 is inexplicable, but by reference to which the explanation of the observed discrepancy is complete; I mean, the old hay which cumbered our laboratory floor.

Four hours' continuous boiling failed to sterilize bulbs charged with infusions of this hay. In special cases, moreover, germs were found so indurated and resistant that five, six, and in one case even eight hours' boiling failed to deprive them of life.

All the difficulties encountered in this long and laborious inquiry were traced to the germs which exhibited the extraordinary powers of resistance here described. They introduced a plague into our atmosphere—the other infusions, those of fresh hay included, like a smitten population, becoming the victims of a contagium foreign to themselves *.

It is a question of obvious interest to the scientific surgeon whether those powerfully resistent germs are amenable to the ordinary processes of disinfection. It is perfectly certain that they resist to an extraordinary extent the action of heat. They have been proved competent to cause infusions, both animal and vegetable, to putrefy. How would they behave in the wards of an hospital? There are, moreover, establishments devoted to the preserving of meats and vegetables. Do they ever experience inexplicable reverses? I think it certain that the mere shaking of a bunch of desiccated hay in the air of an establishment of this character might render the ordinary process of boiling for a few minutes utterly nugatory, thus possibly entailing serious loss. They have, as will subsequently appear, one great safeguard in the complete purgation of their sealed tins of air.

Keeping these germs and the phases through which they pass to reach the developed organism clearly in view, I have been able to sterilize the most obstinate infusions encountered in this inquiry, by heating them for a small fraction of the time above referred to as *insufficient* to sterilize them. The fully developed *Bacterium* is demonstrably killed by a temperature of 140° F. Fixing the mind's eye upon the germ during its passage from the hard and resistant to the plastic and sensitive state, it will appear in the highest degree probable that the plastic stage will be reached by different germs in different times. Some are more indurated than others, and require a longer immersion to soften and germinate. For all known germs there exists a period of incubation during which they prepare themselves for emergence as the finished organisms which have been proved so sensative to heat. If during this period, and well within it, the infusion be boiled for even the fraction of a minute, the softened germs which are then approaching their phase of final development will be destroyed. Repeating the process of heating every ten or twelve hours, before the least *sensible* change has occurred in the infusions, each successive heating will destroy the germs then softened, until, after a sufficient number of heatings, the last living germ will disappear.

Guided by the principle here laid down, and applying the heat discontinuously, infusions have been sterilized by an aggregate period of heating, which, fifty times multiplied, would fail to sterilize them if applied continuously. Four minutes in the one case can accomplish what four hours fail to accomplish in the other.

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* A hard and wiry hay from Guildford, which I have no reason to consider old, was found extremely difficult to sterilize.

If properly followed out, the method of sterilization here described is infallible. A temperature, moreover, far below the boiling-point suffices for sterilization *.

Another mode of sterilization, equally certain and remarkable, was forced upon me, so to speak, in the following way. In a multitude of cases a thick and folded layer of fatty scum, made up of matted *Bacteria*, gathered upon the surfaces of the infusions, the liquid underneath becoming sometimes cloudy throughout, but frequently maintaining a transparency equal to that of distilled water. The living scum-layer, as Pasteur has shown in other cases, appeared to possess the power of completely intercepting the atmospheric oxygen, appropriating the gas and depriving the germs in the liquid underneath of an element necessary to their development.

Placing the infusions in flasks, with large air-spaces above the liquids, corking the flasks, and exposing them for a few days to a temperature of 80° or 90° F., at the end of this time the oxygen of the superjacent air seems completely consumed. A lighted taper plunged into the flask is immediately extinguished. Above the scum, moreover, the interior surfaces of the bulbs used in my experiments were commonly moistened by the water of condensation. Into it the *Bacteria* sometimes rose, forming a kind of gauzy film to a height of an inch or more above the liquid. In fact, wherever air was to be found, the *Bacteria* followed it. It seemed a necessity of their existence. Hence the question, What will occur when the infusions are deprived of air?

I was by no means entitled to rest satisfied with an inference as an answer to this question; for Pasteur, in his masterly researches, has abundantly demonstrated that the process of alcoholic fermentation depends on the continuance of life without air—other organisms than *Torula* being also alleged to be competent to live without oxygen. Experiment alone could determine the effect of exhaustion upon the particular organisms here under review.

Air-pump vacua were first employed, and with a considerable measure of success. Life was demonstrably enfeebled in such vacua.

Sprengel pumps were afterwards used to remove more effectually both the air dissolved in the infusions and that diffused in the spaces above them. The periods of exhaustion varied from one to eight hours, and the results of the experiments may be thus summed up:—Could the air be completely removed from the infusions, there is every reason to believe that sterilization *without boiling* would in most, if not in all, cases be the result. But, passing from probabilities to certainties, it is a proved fact that in numerous cases unboiled infusions deprived of air by five or six hours' action of the Sprengel pump are reduced to permanent barrenness. In a great number of cases, moreover, where the unboiled infusion would have become cloudy, exposure to the boiling temperature for a single minute sufficed completely to destroy the life already on the point of being extinguished through defect of air. With a single exception, I am not sure that any infusion escaped sterilization by five minutes' boiling after it had been deprived of air by the Sprengel pump. These five minutes accomplished what five hours sometimes failed to accomplish in the presence of air.

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* On reading my paper, previous to its presentation to the Royal Society, Mr. Huxley suggested a continuous exposure to a temperature not much above the death-temperature of the adult *Bacterium*. I had also noted the experiment for execution myself. It was made some weeks ago with perfect success. The most obstinate infusions, maintained for a few days at a temperature of 160° F., are sterilized.—[June 21.]

The exception here referred to is old-hay infusion, which, though sterilized in less than half the time needed to kill its germs where air is present, maintained a power of developing a feeble but distinct life after having been boiled for a large multiple of the time found sufficient to render infusions of mutton, beef, pork, cucumber, turnip, beetroot, shaddock, and artichoke permanently barren.

These experiments gave me the clue to many others which might have readily become subjects of permanent misinterpretation. In the midst of a most virulently infective atmosphere, where, even after some hours' boiling, there was no escape for infusions supplied with air, the expulsion of the air by less than five minutes' boiling in properly shaped retort-flasks, and the proper sealing of the flasks during ebullition, ensured the sterility of the infusions.

The meaning of a former remark regarding the part played by boiling in establishments devoted to the preserving of meats and vegetables will be now understood.

The inertness of the germs in liquids deprived of air is not due to a mere *suspension* of their powers. The germs are *killed* by being deprived of oxygen. For when the air which has been removed by the Sprengel pump is, after some time, carefully restored to the infusion, unaccompanied by germs from without, there is no revival of life. By removing the air we stifle the life which the returning air is incompetent to restore.

These experiments on the mortality arising from a defect of oxygen are in a certain sense complementary to the beautiful results of M. Paul Bert. Applying his method to my infusions, I find them sterilized in oxygen possessing a pressure of ten atmospheres or more. Like higher organisms, our Bacterial germs are poisoned by the excess and asphyxied by the defect of oxygen. A mechanical action may also come into play.

A few short sections on *Bacteria* germs as distinguished from *Bacteria* themselves*, and on the alleged destruction of germs by merely drying them, on hermetic sealing, and on the deportment of hermetically sealed flasks exposed to the sun of the Alps, are introduced towards the end of the memoir.

Throughout these laborious researches I have been aided, with his accustomed zeal and ability, by my excellent senior assistant, Mr. John Cottrell. He has been worthily seconded by Mr. Frank Valter, and, in an humbler but still effectual way, by William Card.

May 31, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "The Physical Properties of Homologues and Isomers." By FREDERICK D. BROWN, B.Sc. Communicated by HENRY E. ARMSTRONG, Ph.D., F.R.S., Sec. C.S., Professor of Chemistry in the London Institution. Received April 23, 1877.

Observations of the physical properties of chemical compounds have been very numerous; in many, however, the object aimed at has been merely the definition of the substances in question, and the results lay claim to no great accuracy. In others, data have been required for the solution of purely physical problems, and but little attention has been paid to the chemical nature of the substances employed. In comparatively few has equal importance been attached to both the physical and chemical aspects of the question.

If, then, we attempt to compare the physical properties of a series of compounds presenting very similar chemical properties, we find that in the case of one or two members of the series our knowledge is tolerably complete, but that with regard to the others little is known but their approximate boiling-points and densities.

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* By the excellent researches of Dallinger and Drysdale it has been proved that the germs of Monads, as compared with the adult organisms, possess a power of resistance to heat in the proportion of 11 to 6.

Among the alcohols of the $C_nH_{(2n+1)}.OH$ series, for example, there are two (methyl and ethyl alcohols) with whose physical properties we are well acquainted. The expansion of these bodies has been determined by Kopp, Pierre, Mendelejeff, and others; the vapour-tensions, latent and specific heats by Regnault; the latent heat by Andrews also; the refractive indices by Gladstone and Dale. Nearly all these researches, and many more which might be enumerated, have been accompanied by measurements of density and boiling-point.

When we pass to the other members of the series we find, however, that in the case of normal propyl alcohol, with the exception of numerous determinations of density and boiling-point, experiments have been limited, as far as I am aware, to the measurement of the expansion by Pierre and Puchot. (The table of vapour-tensions given in the same paper is erroneous, as I hope to prove in a future communication.) The same is to be said of all the other alcohols of this series, the rates of expansion of some of which have been examined by Kopp, Lieben and Rossi, Wanklyn and Erlenmeyer, and others.

In order partially to supply this want I have undertaken a series of experiments, of which the following pages are the firstfruits.

I. *Density, Expansion, and Vapour-tension of Propyl and Isopropyl Iodides.*

The selection of these as my first experiments was prompted by the following considerations:—If, as is possible, all the particular physical characteristics of a given substance result from one main characteristic, which is, as it were, the fundamental definition of the substance (at least from a physical point of view), then the various phenomena observed must be dependent one on the other, and the measurement of the vapour-tension, for instance, or of the latent heat, of the given substance must involve implicitly all other measurements, such as those of the specific heat, rate of expansion, &c.; that this is not without foundation, is proved by the many partially successful attempts to connect the various results obtained for certain bodies by mathematical reasoning.

A connexion between the vapour-tension and rate of expansion of a liquid organic body is rendered probable by the well-known researches of Hermann Kopp and others. From a large number of observations conclusions were formed which may be thus summed up:—The atoms of the various elements entering into the composition of substances of the same chemical type always occupy the same space when the tension of the vapour of the liquid is equal to the pressure exerted by a column of mercury 760 millims. in height. Hence the molecules of isomeric substances which differ only in the arrangement of their constituent atoms, and not in their number or kind, must, when the tension equals 760 millims., occupy the same space, or, in other words, the molecular volumes of isomers at their boiling-points are equal. If this relation exists between the molecular volumes of two isomers at those temperatures at which their respective vapour-tensions are equal to 760 millims., it should equally hold good at all those corresponding temperatures at which the vapour-tensions are equal to any number *n* of millimetres, since the ordinary barometric pressure of 760 millims. is a special circumstance due to our residence on this planet and to the usual position of our laboratories at no great altitude.

The study of the vapour-tensions, combined with that of the density and rate of expansion, enables us therefore to compare the molecular volumes at all those corresponding temperatures at which the vapour-tensions are equal; should a uniform equality be apparent, it is evident that a connexion between vapour-tension and rate of expansion would be established. For, let A and a be two isomeric liquids, D and d their respective densities, and H and h their vapour-tensions at any temperature t, we have $D=f(t)$, $d=f'(t)$, $H=\phi(t)$, $h=\phi'(t)$; if we have observed that whenever $H=h$ $D=d$[*], it of course follows that for any two values t_1 and t_2 such that $\phi(t_1)=\phi'(t_2)$ we shall have also $f(t_1)=f'(t_2)$.

Since the expressions $\phi(t)$ and $f(t)$ present no peculiarities in the case of isomeric substances, it would be reasonable to suppose that in the case of other liquids there is also a relation between $\phi(t)$ and $f(t)$.

A detailed description of the apparatus employed, together with all the precautions taken to ensure accuracy, must be reserved for the future; a brief statement on this head is, however, indispensable.

Thermometers.—The two thermometers by which all measurements of temperature were taken are each about 20 inches long, and are divided on the glass into tenths of a degree; the scale of the one (A) ranges from −7° to 53°, that of the other (B) from 46° to 102°. These instruments were most carefully compared with a Kew standard which has long been in the possession of the London Institution.

The bath in which the comparisons were made was 22 inches in height and 12 inches in breadth, and was filled with water which was rapidly and constantly stirred; the readings were marked on paper divided carefully into squares, and the points united by a curved line; the corresponding readings of the air thermometer derived from Regnault's Tables being also marked, the correction for any subsequent reading of either of the instruments could be obtained by direct measurement.

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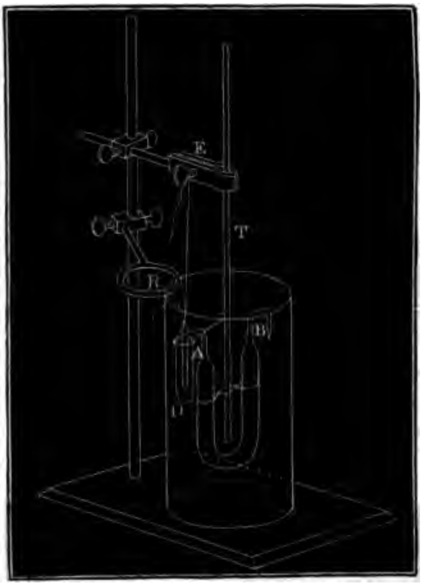
[*] V and v being the respective molecular volumes, and m the common molecular weight, we have $V=\frac{m}{D}$ and $v=\frac{m}{d}$; consequently when $V=v$, $D=d$: we may therefore substitute the equality of the densities for that of the molecular volumes.

Fig. 1.



The error of observation of the temperatures is never greater than 0°·02.

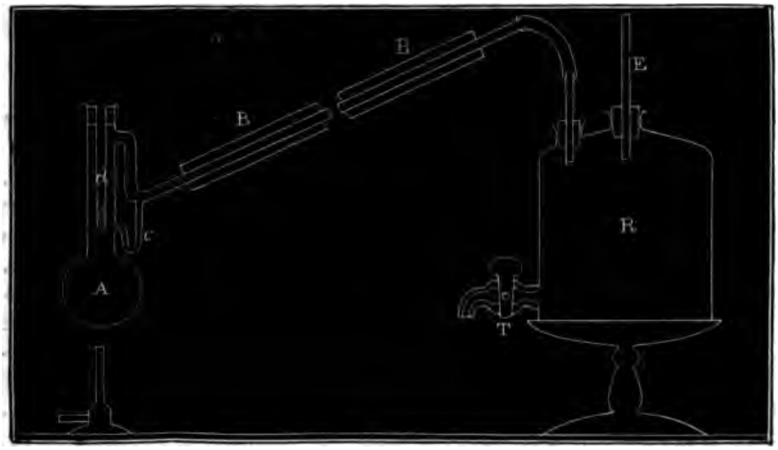
Determination of Densities.—The capillary ends of a U-tube of Dr. Sprengel's pattern, the capacity of which is about 14 cub. centims., were bent down as at A and B; when the tube had been filled with the liquid, and fastened to the thermometer in the position shown in fig. 1, a small tube D, containing a little of the liquid, was suspended so that the end A plunged into it; the tube was then completely filled by sucking with an india-rubber tube at B, and lowered into a large beaker of water, the temperature of which was slightly above that of the room; after stirring frequently for about 15 minutes, the thread supporting the tube D was removed from the clamp E to a fixed ring R. The temperature having been read, the clamp E was finally raised, carrying with it the density-tube completely full. On exposure to the cooler air the liquid in the capillaries quickly recedes, and the tube may be wiped dry and weighed.

This slight modification of the ordinary method enabled me to determine easily the density of a liquid at any temperature above that of the atmosphere, however great might be the rate of expansion. The tube being completely full, the error which in general arises from the adherence of the liquid to empty portions of density-tubes &c. is avoided. The densities are referred to water at zero; the error of the observations is less than 0·00005.

Rate of Expansion.—Dilatometers, similar to those used by Kopp, Pierre, and others, were employed, the necessary data concerning them being obtained in the usual way, slight modifications excepted. The instruments, when filled with the liquid to be examined, were compared with the thermometers A and B, in the large bath mentioned above, in which they were completely immersed; readings were taken both when the temperature of the water was rising and when it was falling. When both dilatometers were filled with the same liquid, the volumes obtained at any given temperature differed only in the fifth decimal place.

Vapour-tension.—The apparatus shown in fig. 2 was used in the case of the iodides. The vapour of the liquid boiling in the bulb A condenses in the tube BB and runs back by the small tube *c*; any cooling of the thermometer by the condensed vapour is thus avoided. R is a reservoir of air with a tap T, with which an air-pump is connected, while the tube E is connected with a pressure-gauge formed of two communicating columns of mercury. The thermometer is placed in the tube *d*, which is closed at the bottom and contains a little mercury.

Fig. 2.



A liquid which is not decomposed by heat below its boiling-point will boil in this apparatus at any temperature and for any length of time, without the thermometer varying one hundredth of a degree. About 100 cub. centims. of the liquid were put in the bulb.

Normal Propyl Iodide.—This substance was prepared by treating pure propyl alcohol with hydriodic-acid gas; the saturated liquid was distilled with steam, the distillate shaken several times with a weak solution of caustic soda, a little iodine added to destroy the phosphorus compounds, again shaken with soda, then with water, and finally dried with phosphoric anhydride. The pure iodide thus obtained amounted to about 1100 grms.; it was carefully fractioned, and after four distillations collected in the following five portions :—

A. Below 102°.
I. 102°·0 to 102°·25.
II. 102°·25 to 102°·5.
III. 102°·5 to 102°·75.
B. Above 102°·75.
} Temperatures not corrected.

The densities of I., II., and III. were as follows :—

I. 1·74772 at 20°·79 ; 1·78684 at 0°.
II. 1·75035 at 19·27 ; 1·78641 at 0.
III. 1·74628 at 20·91 ; 1·78550 at 0.

The densities at zero are calculated by means of the curve of expansion afterwards determined.

For the mean density of zero we have D=1·7863.

The density is given by Chapman and Smith (Chem. Soc. Journ. xxii. 195) as 1·7343 at 16° ; by Rossi (Ann. Chem. und Pharm. clix. 79) as 1·782 at 0° ; by Linnemann (Ann. Chem. und Pharm. clxi. 34) as 1·7610 at 16°; and by Pierre and Puchot (Ann. Chim. et Phys. (4) xxii. 286) as 1·7842 at 0°.

To obtain the rate of expansion, one of the dilatometers was filled with Fraction I. and another with III. The final results were as follows:—

FRACTION I.			FRACTION III.		
No. of Obs.	Temperature (corrected).	Volume.	No. of Obs.	Temperature (corrected).	Volume.
1	0°·0 (ice)	1·00000	2	0°·0 (ice)	1·00000
3	7·06	1·00745	3	7·08	1·00740
3	15·01	1·01601	3	15·07	1·01606
3	23·95	1·02586	3	23·99	1·02593
3	30·99	1·03383	3	31·00	1·03385
2	39·01	1·04312	4	39·09	1·04322
3	44·23	1·04933	3	44·20	1·04930
4	50·41	1·05655	5	50·35	1·05644
3	50·56	1·05672	3	50·45	1·05655
3	57·16	1·06501	3	57·06	1·06479
3	68·07	1·07895	3	67·84	1·07849
3	75·21	1·08830	3	75·11	1·08797
3	80·87	1·09600	3	80·77	1·09558
3	88·70	1·10671	3	88·58	1·10603
5	96·94	1·11836	5	96·74	1·11750

The above results were marked on a large sheet of paper divided by parallel lines into squares, and the points united by means of a curve; the two curves thus obtained being nearly coincident, I judged it unnecessary to repeat the experiment with Fraction II.

The following Table gives the volume of Fraction III. at every ten degrees from 0° to 100°; these numbers are measured direct from the curve, and represent the experiments quite as well as if calculated by means of an equation of the formula

$$V=1+\alpha t+\beta t^2+\gamma t^3.$$

Temperature.	Volume as measured from curves.	Volume given by Pierre.	Temperature.	Volume as measured from curves.	Volume given by Pierre.
0°	1·00000	1·000	60°	1·06852	1·069
10	1·01052	1·010	70	1·08128	1·082
20	1·02142	1·021	80	1·09446	1·096
30	1·03274	1·0324	90	1·10802	1·1105
40	1·04436	1·044	100	1·12200	1·125
50	1·05600	1·056	104·5	....	1·132

The difference between the numbers at the higher temperatures is doubtless partly due to the fact that Pierre measured the density at only four temperatures, viz. 0°, 9°·1, 52°·6, and 75°·3, and then calculated the above series of numbers by means of a formula of interpolation; it will be observed that a small error in the determination of the density at 75°·3 would be sufficient to cause the discrepancy referred to.

The vapour-tension of the normal iodide was determined by boiling in the apparatus described above; here, again, Fractions I. and III. only were employed. The following Table gives the results obtained: the observations marked thus * were made with platinum foil in the boiling liquid; with the others this was not the case.

Fraction I.			Fraction II.		
No. of Obs.	Boiling-point.	Pressure in millims.	No. of Obs.	Boiling-point.	Pressure in millims.
3	52°·66	138·9	2	53°·06	138·6
2	59·86	184·7	1	53·17	139·8*
1	62·95	208·0	1	53·26	139·6
1	72·12	289·9	1	56·17	157·9*
1	83·32	424·9	3	61·80	195·9
1	93·85	591·9	1	62·70	203·0*
1	101·14	735·0	1	70·45	269·6
1	102·00	753·4	1	79·11	365·3
			1	84·15	431·3*
			1	90·05	520·0
			1	96·81	640·4
			1	100·21	707·1
			1	100·45	712·7*
			1	101·37	732·1

I attempted to measure the vapour-tensions of this substance in an apparatus similar to that now used for the analysis of gases, one of the vertical tubes, however, being heated in a large quantity of water, which was made to circulate by means of a screw. This apparatus (which will be described on another occasion) answers perfectly in the case of most substances, but both the iodides now under consideration are decomposed by the mercury; the normal iodide, however, being but slowly acted upon in the cold, I was able to measure the vapour-tension several times at about 7°. The mean result shows a tension of 15·4 millims. at 7°·5.

The curves constructed by means of the above data diverge gradually until at a tension of 760 millims., the corresponding temperatures differing by 0°·5. The equation $H = ab^{\frac{t}{1+mt}}$ (where H = the tension of the vapour, t = the temperature in degrees Centigrade +7°·5) expresses the observations for Fraction III. with great exactitude when

$$\log a = 1{\cdot}1875207,$$
$$\log b = 0{\cdot}0259870,$$
$$\log m = \bar{3}{\cdot}6338070.$$

Isopropyl Iodide.—This body was prepared by heating glycerin with iodine and phosphorus, and purifying as in the case of the normal iodide; more than a kilogramme of the pure substance was obtained. After fractional distillation three portions were obtained boiling as follows:—

$$\left.\begin{array}{r} \text{I. } 88^{\circ}{\cdot}6 \text{ to } 88^{\circ}{\cdot}7 \\ \text{II. } 88{\cdot}7 \text{ to } 88{\cdot}8 \\ \text{III. } 88{\cdot}8 \text{ to } 89{\cdot}0 \end{array}\right\} \begin{array}{c}\text{Temperatures} \\ \text{uncorrected.}\end{array}$$

The densities of these three fractions were as follows:—

I. 1·70526 at 19°·80; 1·74315 at 0°
II. 1·70506 at 20·14; 1·74363 at 0
III. 1·70457 at 21·09; 1·74504 at 0.

The mean density at 0° is therefore

$$D = 1{\cdot}7440.$$

The density of isopropyl iodide is given by Erlenmeyer (Ann. Chem. Pharm. cxxvi. 309) as 1·714 at 16°; by Linnemann (Ann. Chem. Pharm. cxxxix. 229) as 1·735 at 0°; by Buff (Ann. Chem. Pharm. Supp. iv. 129) as 1·71732 at 17°.

The observations on the expansion were as follows:—

Fraction II.			Fraction III.		
No. of Obs.	Temperature (corrected).	Volume.	No. of Obs.	Temperature (corrected).	Volume.
1	0°·0 (ice)	1·00000	1	0°·0 (ice)	1·00000
3	14·79	1·01653	1	11·28	1·01263
3	25·81	1·02933	2	15·58	1·01751
2	35·61	1·04110	2	21·84	1·02475
2	39·51	1·04587	2	27·78	1·03178
3	52·28	1·06200	2	32·64	1·03756
3	57·88	1·06947	3	46·97	1·05513
3	70·14	1·08633	3	55·22	1·06591
			3	63·16	1·07662
			3	71·87	1·08865
			4	79·66	1·09994

The two curves drawn by means of these data coincided throughout. The volumes at every ten degrees are as follows :—

Temperature.	Volume.	Temperature.	Volume.	Temperature.	Volume.
0°	1·00000	40°	1·04640	70°	1·08606
10	1·01118	50	1·05900	80	1·10032
20	1·02246	60	1·07234	90	1·11480
30	1·03440				

The following are the results obtained when these two fractions were boiled at various pressures :—

Fraction II.			Fraction III.		
No. of Obs.	Boiling-point.	Pressure in millims.	No. of Obs.	Boiling-point.	Pressure in millims.
1	49°·32	191·9	2	48°·90	187·9
1	52·15	214·3	1	54·43	232·6
2	59·45	281·3	3	55·93	245·9
2	69·55	400·3	3	62·40	311·2
1	73·46	455·6	1	69·63	399·7
1	76·21	498·4	1	72·30	437·4
1	82·17	603·1	1	74·71	472·8
1	86·11	679·6	3	78·13	530·6
1	88·04	720·3	1	80·52	570·6
1	89·81	760·2	3	83·79	631·9
			1	87·64	709·9
			1	89·66	754·8

The two curves are in this case almost identical. A satisfactory equation has not yet been obtained owing to the absence of any measurements at low temperatures.

In the following Table the densities are compared at those temperatures at which the vapour-tensions are equal:—

Vapour-tension in millims.	Boiling-point of normal propyl iodide.	Boiling-point of iso-propyl iodide.	Density of normal propyl iodide.	Density of iso-propyl iodide.	Difference between densities.
200	62°·37	50°·50	1·66704	1·64590	0·02114
300	73·51	61·33	1·64493	1·62359	0·02134
400	81·95	69·70	1·62808	1·60646	0·02162
500	88·84	76·44	1·61446	1·59246	0·02200
600	94·70	82·11	1·60250	1·58068	0·02182
700	99·83	87·13	1·59221	1·57035	0·02186
760	102·63	89·86	1·58670	1·56497	0·02196

The above numbers show that the relation between vapour-tension and expansion, which had been rendered probable by Kopp's researches, does not exist, that the molecular volumes of the two isomers at corresponding temperatures are unequal, that of the normal iodide being greater than that of the other by an almost constant quantity. This fact is in accordance with the modern dynamical hypotheses on the constitution of matter. Isopropyl iodide is more unstable than the normal iodide; its instability may be due to the greater velocity of its molecules; from this greater velocity would follow a greater mean distance between the molecules.

I may here mention that I have made a very complete series of experiments on the vapour-tension of normal propyl alcohol. The curve representing these observations intersects that which expresses the tensions of normal propyl iodide, so that whereas at 760 millims. the iodide boils at 102°·5 and the alcohol at 97°·3, at 370 millims. they boil at the same temperature, viz. 79°·5, and at 120 millims. the boiling-point of the iodide is only 49°·5, whilst that of the alcohol is 56°.

This fact, which probably arises from the much greater latent heat of propyl alcohol, obviously renders useless all attempts to derive the boiling-points from the constitution of chemical compounds, so long as the boiling-points at the ordinary pressure of the atmosphere alone are taken into account.

II. "On the Amplitude of Sound-waves." By Lord RAYLEIGH, M.A., F.R.S. Received May 3, 1877.

Scarcely any attempts have been made, so far as I am aware, to measure the actual amplitude of sound-bearing waves, and, indeed, the problem is one of considerable difficulty. Even if the measurement could be effected, the result would have reference only to the waves actually experimented upon, and would be of no great value in the absence of some means of defining the intensity of the corresponding sound. It is bad policy, however, to despise quantitative estimates because they are rough; and in the present case it is for many reasons desirable to have a general idea of the magnitudes of the quantities with which we have to deal. Now it is evident that a superior limit to the amplitude of waves giving an audible sound may be arrived at from a knowledge of the energy which must be expended in a given time in order to generate them, and of the extent of surface over which the waves so generated are spread at the time of hearing. An estimate founded on these data will necessarily be too high, both because sound-waves must suffer some dissipation in their progress, and also because a part, and in some cases a large part, of the energy expended never takes the form of sound-waves at all.

The source of sound in my experiment was a whistle, mounted on a Wolf's bottle, in connexion with which was a siphon manometer for the purpose of measuring the pressure of wind. This apparatus was inflated from the lungs through an india-rubber tube, and with a little practice there was no difficulty in maintaining a sufficiently constant blast of the requisite duration. The most suitable pressure was determined by preliminary trials, and was measured by a column of water 9½ centimetres high.

The first point to be determined was the distance from the source to which the sound remained clearly audible. The experiment was tried in the middle of a fine still winter's day, and it was ascertained that the whistle was heard without effort at a distance of 820 metres. In order to guard against any effect of wind, the precaution was taken of repeating the observation with the direction of propagation reversed, but without any difference being observable.

The only remaining datum necessary for the calculation is the quantity of air which passes through the whistle in a given time. This was determined by a laboratory experiment. The india-rubber tube was put into connexion with the interior of a rather large bell-glass open at the bottom, and this was pressed gradually down into a large vessel of water in such a manner that the manometer indicated a steady pressure of 9½ centimetres. The capacity of the bell-glass was 5200 cubic centimetres, and it was found that the supply of air was sufficient to last 26½ seconds of time. The consumption of air was therefore 196 cubic centimetres per second.

In working out the result it will be most convenient to use consistently the C.G.S. system. On this system of measurement the pressure employed was $9\frac{1}{2}\times981$ dynes per square centimetre, and therefore the work expended per second in generating the waves was $196\times9\frac{1}{2}\times981$ ergs. Now the mechanical value of a series of progressive waves is the same as the kinetic energy of the whole mass of air concerned, supposed to be moving with the maximum velocity of vibration (v); so that, if S denotes the area of the wave-front considered, a be the velocity of sound, and ρ be the density of air, the mechanical value of the waves passing in a unit of time is expressed by $\frac{1}{2}S\,.\,a\,.\,\rho\,.\,v^2$, in which the numerical value of a is about 34100, and that of ρ about ·0013. In the present application S is the area of the surface of a hemisphere whose radius is 82000 centimetres; and thus, if the whole energy of the escaping air were converted into sound, and there were no dissipation on the way, the value of v at the distance of 82000 centimetres would be given by the equation

$$v^2=\frac{2\times196\times9\frac{1}{2}\times981}{2\pi(82000)^2\times34100\times\cdot0013},$$

whence $v=\cdot0014$ centimetre per second.

This result does not require a knowledge of the pitch of the sound. If the period be τ, the relation between the maximum excursion x and the maximum velocity v is

$$x=\frac{v\tau}{2\pi}.$$

In the present case the note of the whistle was f^{iv}, with a frequency of about 2730. Hence

$$x=\frac{\cdot0014}{2\pi\times2730}=10^{-8}\times8\cdot1,$$

or the amplitude of the aerial particles was less than a ten-millionth of a centimetre.

I am inclined to think that on a still night a sound of this pitch, whose amplitude is only a hundred-millionth of a centimetre, would still be audible.

III. "On the alleged Correspondence of the Rainfall at Madras with the Sun-spot Period, and on the True Criterion of Periodicity in a series of Variable Quantities." By General STRACHEY, R.E., C.S.I., F.R.S. Received May 3, 1877.

A paper has recently been printed by Dr. Hunter, the Director-General of Statistics to the Government of India, having for its object to show that the records of the rainfall at Madras, for a period extending over sixty-four years, establish a cycle of rainfall at that place which has a marked coincidence with a corresponding cycle of sun-spots—the rainfall and sun-spots attaining a minimum in the eleventh, first, and second years, and a maximum in the fifth year.

Irrespective of its scientific interest, the conclusion thus adopted would, if sound, be of no little practical importance, as it would supply a means of indicating the probable recurrence of those seasons of excessive drought which produce such terrible results in India, and from one of which the Madras provinces are now suffering in an extreme degree.

It is probably generally known that the conclusion which it has thus been stated is to be drawn from the Madras observations had been considered to be established some years ago, in a more general manner, by Mr. Meldrum, the Director of the Meteorological Observatory at Mauritius, a paper by whom on this subject was read before the Royal Society in 1873, and may be found in vol. xxi. of the 'Proceedings,' p. 297.

As the numerical results of the method of treating the rainfall observation which Mr. Meldrum and Dr. Hunter have followed at first sight may appear to support the conclusions they have adopted, it has seemed to me desirable to examine the facts, with a view to arriving at an independent opinion as to the trustworthiness or otherwise of those conclusions. I shall first refer to the Madras observations and Dr. Hunter's results.

The Madras register extends over sixty-four years, beginning with 1813. The mean rainfall for the whole period is 48·5 inches. The deviations from the mean vary from 30·1 ins. in defect to 39·9 ins. in excess. The arithmetical mean of these deviations (disregarding the signs) is 12·4 ins. The greatest difference between two consecutive years is 50·5 ins., and the average difference 15·8 ins.

Dr. Hunter, in order to test the point which he proposes to investigate, divides the sixty-four years' observations into six cycles of eleven years, and calculates the arithmetical mean of the successive years of the whole series of cycles. The results are shown in the following Table, the figures in which represent the differences of the several years' observations from the mean of the whole:—

TABLE I.

Number of cycle.		Years of cycle.										
		1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.	11th.
		in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
Original observations.	1st cycle	− 3·4	−16·1	+ 7·5	− 7·4	+15·1	+27·7	−12·2	+21·5	− 1·4	+11·1	−21·9
	2nd "	−14·8	+ 7·5	+12·2	+39·9	−10·6	−11·6	−16·1	− 4·2	−30·1	−11·4	− 9·5
	3rd "	− 7·0	− 3·8	+ 0·8	+ 3·8	+ 4·6	+10·1	+ 9·8	−12·0	+ 1·8	+16·9	−10·5
	4th "	+31·3	+32·5	+ 6·3	− 8·7	−11·6	+15·8	+24·2	−12·7	− 5·3	−16·2	− 1·5
	5th "	+ 4·4	0	+ 6·6	−20·9	−11·3	−10·3	+ 6·1	− 1·3	− 6·9	+ 2·9	−24·1
	6th "	− 7·1	−16·2	+25·6	+ 7·8	+25·2	+ 3·3	+14·4	−11·4	−27·0		
Mean difference from the mean of 64 years ...		+ 0·6	+ 0·7	+ 9·8	+ 2·4	+ 1·9	+ 5·8	+ 4·4	− 3·4	−11·5	+ 0·7	−13·5

In the above calculation the first year of the cycle of eleven is 1813, so that the average period of maximum sun-spots will be about the third or fourth year of the cycle, and the period of minimum will be about the tenth or eleventh of the cycle. This Table certainly seems to indicate a period of maximum between the third and the seventh years, and of minimum between the eighth and the second years.

But to estimate the true weight of these results we must look a little deeper. Now the only signification of the arithmetical mean of a series of observed quantities is that it is a quantity above and below which there is an equal amount of deviation in the aggregate of individual observations. Further, conformity to a law of any sort in a series of observations in relation to quantity is obviously to be tested by the extent of deviation of the observed quantities from the results that such law requires. Consequently, in the present case, the question whether or not the mean values shown in Table I. can be accepted as showing a definite law of variation from year to year in the cycle must be determined by examining the differences between those means and the individual observations on which they are based.

Treating the observations in this manner, the following results are obtained :—

TABLE II.

Number of cycle.		Years of cycle.										
		1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.	11th.
		in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
Differences of mean values in Table I. from individual observations.	1.	– 4·0	–16·8	– 2·3	– 9·8	+13·2	+21·9	–16·6	+24·9	+10·1	+10·4	– 8·4
	2.	–15·4	+ 6·8	+ 2·4	+37·5	–12·5	–17·4	–20·5	– 0·8	–18·6	–12·1	+ 4·0
	3.	– 7·6	– 4·5	– 9·0	+ 1·4	+ 2·7	+ 4·3	+ 5·4	– 8·6	+13·3	+16·2	+ 3·0
	4.	+30·7	+31·8	– 3·5	–11·1	–13·5	+10·0	+19·8	– 9·3	+ 6·2	–16·9	+12·0
	5.	+ 3·8	– 0·7	– 3·2	–23·3	–13·4	–16·1	+ 1·7	+ 2·1	+ 4·6	+ 2·2	–10·6
	6.	– 7·7	–16·9	+15·8	+ 5·4	+23·3	– 2·5	+10·0	– 8·0	–15·5		
Mean difference, irrespective of sign		11·5	12·9	6·0	14·8	13·1	12·0	12·3	9·0	11·4	11·6	7·6

Mean of mean differences 11·2 inches.

Thus it appears that the mean difference of the individual observations from the calculated means shown in the last line of Table I. differs but little from the mean difference of the individual observations from the arithmetical mean of the whole series. In other words, the supposed law of variation obtained from the means of the six 11-year cycles hardly gives a closer approximation to the actual observations than is got by taking the simple arithmetical mean as the most probable value for any year.

In order to obtain a practical test of the probable physical reality of the cycle of eleven years, I have calculated a series of mean values corresponding to those given in Table I. for a series of cycles of five, six, seven, eight, nine, ten, twelve, and fourteen years. I find that the mean differences between these means and the observed quantities, and therefore corresponding to the mean differences shown in the last line of Table II., are all within a very small fraction of one another and of the mean obtained from the 11-year cycle—in short, that one cycle is in this respect almost as good or as bad as another.

The mean differences for the several cycles are given in the following Table:—

TABLE III.

Cycles of	Years of the cycle.														Mean.
	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th	9th.	10th.	11th.	12th.	13th.	14th.	
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
5 years ...	9·0	9·5	12·8	10·6	18·3	...	...	...	...	...	...	...	...	...	11·9
6 years ...	7·7	13·2	11·5	14·7	13·2	13·3	...	...	...	...	...	...	...	...	12·2
7 years ...	15·9	8·9	6·7	12·4	8·5	19·2	12·5	...	...	...	...	...	...	...	12·1
8 years ...	5·2	15·0	12·1	11·8	8·0	11·3	14·9	16·3	...	...	...	...	...	...	11·8
9 years ...	10·6	11·2	10·9	12·6	11·3	16·7	10·7	13·2	7·0	...	...	...	...	...	11·6
10 years ...	8·5	10·5	7·8	12·4	17·7	9·3	7·9	17·5	8·4	18·8	...	...	...	...	11·7
11 years ...	11·5	12·9	6·0	14·7	13·1	12·0	12·3	9·0	11·4	11·6	7·6	...	...	...	11·2
12 years ...	5·8	11·4	11·6	13·0	11·0	12·7	7·8	15·4	6·8	13·4	14·4	13·9	...	...	11·4
14 years ...	17·7	12·0	4·5	13·9	7·5	24·0	14·1	14·2	5·1	6·3	11·3	10·1	12·2	8·7	11·9

Now if in any series of quantities, such as the rainfall observations at Madras, there be a law of periodicity, each observed quantity may be supposed to be compounded of a periodical and a non-periodical element. If we take the sum of a large number of cycles each of which coincides with the cycle of periodicity, the non-periodical elements will tend to occur in equal amount in excess and defect, and thus to be eliminated, and the means for the successive years of the cycle, or whatever the intervals be (which I will term cyclical means), will tend to indicate the periodical elements for the successive intervals. At the same time, the differences of these cyclical means from the several original quantities from which they were obtained will approximate to the several non-periodical elements. These differences I will call cyclical differences.

In proportion as the periodical elements are small or large in relation to the corresponding non-periodical elements, so the cyclical differences will be inversely less or more different from the differences between the individual observations and the mean of the whole of them; and if there be no periodicity, the cyclical means will tend to disappear, and the two sets of differences would, in a sufficiently long series, be identical.

Hence it may be inferred that when the cyclical differences closely approximate in magnitude to the mean difference of the original observations from the arithmetical mean of all of them, the periodical elements in those observations must be correspondingly small; and this applies manifestly to the whole of the cycles for which the differences are shown in Table III.

Further to test the reality of the supposed periodicity shown in Table I., I have rearranged the series of 64 years' observations, in a purely arbitrary manner, in cycles of eleven years, by drawing the actual observations at random one after another, and setting them down in succession till the whole were exhausted. From three arbitrary sets of six cycles thus prepared the mean cyclical difference averaged 10·9, 11·2, and 11·6—results which again indicate that, by adopting the actual sequence of the observed quantities of rain instead of taking them at random, we produce no material effect on the mean cyclical differences, nor any such tendency to a diminution in their numerical value as necessarily accompanies a true periodical element.

It is, moreover, important to bear in mind that the mere circumstance of any series of cyclical means showing a single maximum and single minimum gives no more real indication that such a result is a truly periodical feature than would be supplied by the appearance of two or more maxima and minima. The law of periodicity, if it exist at all, can only be inferred by the facts indicated by observation; and it is obviously to argue in a circle, first to assume a cycle on which to work, which shall give a single maximum and minimum, and then to infer that there is true periodicity because of the single maximum and minimum. The test of the periodicity is in truth to be sought altogether outside of the particular values of the successive cyclical means.

It is, of course, manifest that a complication of periodical elements may so mask one another as to prevent positive results being obtained by such an examination of the cyclical means and differences as I have made in the case before us. But the whole scope of my present argument is negative, and it necessarily leads, I think, to the conclusion that the cyclical variations shown in Table II. from the mean values in Table I. are so great as to show that any apparent regularity or tendency to a maximum in one part of the 11-year cycle, and a minimum in another, has no real weight, and that there is no proof of greater tendency to periodicity in the 11-year means than in the original isolated observations.

This might perhaps be considered all that need be said on this subject but an objection may possibly be made to the effect that the sun-spot period is not exactly a cycle of eleven years, and that a better result may be obtained by a comparison of the observations corresponding to the known periods of maximum and minimum sun-spots, without reference to any special length of cycle. The following Tables show the results thus arrived at, the figures representing differences from the mean rainfall for the whole sixty-four years.

TABLE IV.

Rainfall near periods of maximum sun-spots.						Rainfall near periods of minimum sun-spots.					
Years.	Two years before.	One year before.	Maximum.	One year after.	Two years after.	Years.	Two years before.	One year before.	Minimum.	One year after.	Two years after.
	in.	in.	in.	in.	in.		in.	in.	in.	in.	in.
1817.	+ 7·5	− 7·4	+15·1	+27·7	−12·2	1823.	− 1·4	+11·1	−21·9	−14·8	+ 7·5
1830.	−10·6	−11·6	−16·1	− 4·2	−30·1	1833.	− 4·2	−30·1	−11·4	− 9·5	− 7·0
1837.	− 7·0	− 3·8	− 0·8	+ 3·8	+ 4·6	1843.	+ 9·8	−12·0	+ 1·8	+16·9	−10·5
1849.	+32·5	+ 6·3	− 8·7	−11·6	+15·8	1856.	− 5·3	−16·2	− 1·5	+ 4·4	− 0·0
1860.	− 0·0	+ 6·6	−20·9	−11·3	−10·3	1867.	− 6·9	+ 2·9	−24·1	− 7·1	−16·2
1871.	−16·2	+25·6	+ 7·8	+25·2	+ 3·3						
Mean...	+ 1·0	+ 2·6	− 4·0	+ 4·9	− 4·8	Mean...	− 1·6	− 8·9	−11·4	− 2·0	− 5·2
Mean difference from mean	12·6	10·2	10·0	14·3	12·7	Mean difference from mean	4·6	12·7	9·3	10·1	7·2
Mean of mean differences 12·0 inches.						Mean of mean differences 8·8 inches.					

These results are also negative. The maximum period cannot be said to be indicated at all. The minimum shows a diminished mean cyclical deviation, but this is due to the years furthest from the supposed minimum epoch; and if the three central years alone are reckoned, the result is much the same as obtained from the 11-year cycles.

I have sought for a further test of the character of the conclusions that I have been discussing in the rainfall observations at Bombay and Calcutta, which have been made for the greater part of the period over which those at Madras extend. It is hardly conceivable that there should be a coincidence with the sun-spot period, such as is supposed to have been found at Madras, based on any physical cause, which should not in some way be discernible in the rainfall at Bombay and Calcutta. Adopting the same 11-year cycle for the observations at these two places as was used in the case of Madras, the mean results for the three localities are exhibited in the following Table:—

Table V.

	No. of years' observations.	Mean rainfall of whole period.	Average difference between mean rainfall of whole period of observation and means of each year of cycle of eleven years.										
			1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.	11th.
		in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
adras ...	64	48·5	+ 0·6	+0·7	+9·8	+2·4	+ 1·9	+5·8	+4·4	−3·4	−11·5	+ 0·7	−13·5
ombay...	52	76·9	0·0	+4·1	−1·1	−2·4	+11·2	+2·9	+1·7	+5·4	− 5·8	−10·8	− 9·4
alcutta...	47	65·8	+14·8	−5·2	−8·0	+1·5	+ 4·6	−5·3	−0·3	+0·0	− 5·0	+ 1·8	+ 1·8

These results are entirely negative, and indicate no concordance among the means of the several years of the cycle. The Bombay and Calcutta observations, treated as those of Madras were, to ascertain the deviations of individual observations from the successive means of the cycle, give quite similar results. The deviations obtained for Bombay and Calcutta, in the manner shown in Table II., are as follows:—

Table VI.

	Average differences irrespective of sign between separate observations and means of each year of cycle of eleven years.											Mean of mean differences.
	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.	11th.	
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
Bombay	7·1	16·6	5·5	20·4	20·8	13·6	6·7	15·3	9·6	15·8	6·4	12·7
Calcutta	7·9	6·6	7·1	14·6	12·3	8·4	7·9	14·3	5·0	4·5	5·1	8·0

In these cases, as in that of Madras, the mean deviation for the whole eleven years of the cycle differs very little from the mean variation of the single observations from the arithmetical mean of all of them, these quantities being for Bombay 13·4 in., and for Calcutta 9·0 in.

Although my special object in the present communication is to deal with the alleged correspondence between the Madras rainfall and the sun-spot periods, I have naturally turned my attention to Mr. Meldrum's speculations of a similar character, and I have tested some of them in the manner that I have just explained.

Taking the Greenwich observations for fifty-five years, which will be found at page 307 of vol. xxi. of the 'Proceedings of the Royal Society,' in Mr. Meldrum's paper before noticed, and arranging them in the manner shown in Table IV., the following results are obtained. The mean rainfall for the whole period is 24·9 inches, and the entries are the differences from this mean.

TABLE VII.

Rainfall near periods of maximum sun-spots.						**Rainfall near periods of minimum sun-spots.**					
Year.	**Two years before.**	**One year before.**	**Maxi-mum.**	**One year after.**	**Two years after.**	**Year.**	**Two years before.**	**One year before.**	**Mini-mum.**	**One year after.**	**Two years after.**
	in.	in.	in.	in.	in.		in.	in.	in.	in.	in.
1817	− 4·4	+2·6	+1·7	−1·5	+3·3	1823	+6·6	+0·1	−0·5	+8·1	−2·6
1830	+ 3·5	−2·3	+6·2	+9·6	+4·8	1834	+4·8	+4·0	−1·1	+5·7	+6·1
1837	+ 5·7	+6·1	−5·8	−3·3	+2·2	1844	−4·9	−2·4	−3·0	−6·2	−1·4
1849	−12·1	+5·3	−1·0	−5·2	−3·3	1856	−5·9	−1·1	−3·0	−3·5	−7·1
1860	− 7·1	+1·0	+7·1	−4·5	+1·6	1867	+3·8	+5·8	+3·5	+0·2	−0·9
Mean...	− 2·9	+2·5	+1·6	−1·0	+1·7	**Mean ...**	+0·9	+1·3	−0·8	+4·3	−1·2
Mean difference from mean	6·0	2·5	4·0	4·2	2·1	**Mean difference from mean**	5·0	2·9	1·9	4·8	3·0
Mean of mean differences 3·8 inches.						**Mean of mean differences 3·5 inches.**					

In this case the mean deviation of the original observations from the arithmetical mean of the whole series (24·9 inches) is 4·1 inches. This result therefore is quite analogous to that obtained from the Indian observations.

I have not attempted to make detailed calculations in other cases, but, so far as I can judge, the evidence of the alleged periodicity will be generally found to fail when it is tested by comparison with the individual observations on which it has been made to rest.

It will serve to illustrate the argument on which this paper is based if we consider what would be the consequence of applying it to a case in which a well-ascertained periodicity exists, as that of the diurnal barometric oscillation. The following Table gives an example, taken at random from an old Madras register, the intervals being made two-hourly, so as to reduce the number of calculations. The entries are the differences of the observed barometric heights from the mean of the whole in thousandths of an inch.

TABLE VIII.

Cycles of one day.	Intervals of cycle.—Two hours.											
	0.	2.	4.	6.	8.	10.	12.	14.	16.	18.	20.	22.
1st cycle............	−38	− 5	+53	+83	+45	+10	+32	+52	+78	+54	− 6	−59
2nd „	−87	−58	−21	+13	−11	−12	− 9	+ 1	+16	+ 5	−32	−72
3rd „	−72	−43	+ 2	+19	− 2	−16	−13	+21	+57	+42	− 2	−45
4th „	−63	−28	+18	+47	+22	− 7	+ 4	+20	+64	+37	−13	−46
5th „	−52	−29	+ 9	+24	+ 7	−21	− 3	+35	+56	+35	−13	−74
Mean difference from mean of 60 obs..........	−62	−33	+12	+37	+12	− 9	+ 2	+26	+54	+35	−13	−59

In this case it will be found that the mean deviation (disregarding sign) of the whole of the observations from the arithmetical mean of the whole (which is here zero) will be 30·3.

The differences between the mean values, given in the last line of Table VIII., for the two-hourly periods and the individual observations are as follows:—

TABLE IX.

Cycles of one day.	Intervals of two hours.											
	0.	2.	4.	6.	8.	10.	12.	14.	16.	18.	20.	22.
1st cycle............	+24	+28	+41	+46	+33	+19	+30	+26	+24	+19	+ 7	0
2nd „	−25	−25	−33	−24	−23	− 3	−11	−25	−38	−30	−19	−13
3rd „	−10	−10	−10	−18	−14	− 7	−15	− 5	+ 3	+ 7	+11	+14
4th „	− 1	+ 5	+ 6	+10	+10	+ 2	+ 2	− 6	+10	+ 2	0	+13
5th „	+10	+ 4	− 3	−13	− 5	−12	− 5	+ 9	+ 2	0	0	−15
Mean difference, disregarding the signs	14	14	19	22	17	9	13	14	15	12	7	11
Mean of mean differences 14.												

Here a true periodicity existing, the mean of the differences, or, as I before called it, the cyclical deviation, is reduced to 14. Moreover the several differences for the separate observations for the successive hours are well below the mean variation 30, and none of the separate results is so high as that figure.

If we had such a series of sixty observations as is contained in

Table VIII., and had no knowledge of the law of periodicity, and hypothetically applied a period of ten intervals instead of twelve (following the process the results of which for Madras are given in Table III.), the inapplicability of such a period would immediately be made apparent by results such as are found in the rainfall observations. On the hypothesis just mentioned the mean values, corresponding to those given in the last line of Table VIII., for the successive intervals of the six arbitrary cycles that would be formed would be as follows:—

Table X.

Cycles of 20 hours.	Intervals of two hours.									
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Mean difference from mean of 60 obs..........	−2	−3	0	−1	+2	+1	+1	+3	+3	−3

From these figures the following differences would be deduced instead of those shown in Table IX.

Table XI.

Cycles of 20 hours.	Intervals of two hours.									
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
1st period	36	1	53	84	43	9	31	49	75	57
2nd "	4	55	87	57	23	12	12	15	12	4
3rd "	18	9	32	71	74	44	1	16	5	13
4th "	11	25	57	43	4	46	64	31	15	50
5th "	24	3	4	21	62	36	14	49	55	26
6th "	11	28	7	20	5	34	55	32	16	71
Mean difference, disregarding signs............	17	20	40	49	35	30	30	32	30	37
Mean of mean differences 32.										

The contrast between these results and those given in Table IX. is most striking, and calls for no comment in detail.

In conclusion I would explain that I do not desire to call in question the possible or actual occurrence of periodical terrestrial phenomena corresponding to the sun-spot period, but to point out in the case of the rainfall not only has no such correspondence been established, but that there has been no sufficient evidence adduced of any periodicity at all.

Appendix.—Received May 17, 1877.

Suppose the numerical values of such a series of observations as that discussed in the preceding paper to be represented by

$A_1, A_2, A_3, \ldots$ for the first year of each cycle of (n) years,
$B_1, B_2, B_3, \ldots$ for the second years,
and so on for (m) cycles.

Also suppose M to represent the arithmetical mean of all the observations, and $p_a, p_b, p_c, \ldots$ to be the periodical variations from the mean, M, for the several series of the A, B, ... years of the cycles, and the corresponding non-periodical variations to be $a_1, a_2, a_3, \ldots, b_1, b_2, b_3, \ldots,$ and so forth—all these quantities being affected by their proper signs.

Then if M_a represents the mean value of any series $A_1, A_2, A_3, \ldots,$ we should have for (m) cycles—

$$M_a = M + p_a + \frac{1}{m}\{a_1 + a_2 + \ldots\} = M + p_a + e_a,$$

where e_a with its proper sign represents the non-periodical portion of the mean value of the A series for (m) years.

With a sufficiently prolonged series of cycles the quantities $a_1, a_2, \ldots$ will tend to cancel one another and e_a will disappear; so that M_a will then become equal to $M + p_a$, and $M_a - M = p_a$.

If, therefore, there is a truly periodical element, the difference of the mean of all the observations, and of the mean of any series (A) of the cycle of periodicity, will (in a sufficiently extended series of observations) tend to be identical with the periodical element (p_a) for that cycle.

This holds good of all the series A, B, C, ...; and therefore the sum of all the differences last mentioned for the several series will tend to equality with the sum of all the periodical elements of the several series. This will be true whether we regard the algebraic sign or not. If we so regard it, the sum of the differences will evidently become equal to zero, as also will the sum of the periodical elements. If we disregard the signs, the sums will have a numerical value, which call S. This, if divided by (n), the number of years in the cycle of periodicity, will indicate the mean deviation, either in excess or defect, of the periodical elements from the arithmetical mean of all the observations.

Hence, when there is a true periodicity, the sum of these differences taken without regard to sign tends to become an invariable quantity, and the numerical magnitude of this quantity indicates the magnitude of the periodical variation.

Let us next consider the case in which there is no truly periodical element. Here $M_a = M + e_a$ and $M_a - M = e_a$. If E_a represents the mean numerical value, irrespective of sign, of all the deviations of an indefinitely prolonged series of observed quantities $A_1, A_2, \ldots$ from their mean M_a, then the probable value of e_a deduced from the mean of (m) cycles, or series of observations of A, also irrespective of sign, will (according to the known laws of the combination of errors) equal $\frac{E_a}{\sqrt{m}}$. The same will hold good of all the series B, C, &c. At the same time, as there is no periodicity, and all the observations are presumably liable to errors or irregularities of the same general character in a positive or negative direction, the quantities E_a, E_b, &c. will, in a sufficiently prolonged series, tend to equality one with another and also with the mean deviation, irrespective of sign, of all the observed quantities from the arithmetical mean of the whole of them, which call E.

Hence, when there is no periodicity, the sum S, as before defined, tends to become $n \times \frac{E}{\sqrt{m}}$.

We are thus led to the conclusion that the consideration of the successive values of the quantity S as the number of cycles of periodicity increases affords a true criterion of the presence or absence of a periodical element. If as (m) increases this quantity is gradually reduced in a ratio approximating to $\frac{E}{\sqrt{m}}$, we may infer that the periodicity is small or does not exist. On the other hand, if the value of S tends to become invariable, and continues to be of considerable numerical magnitude after a prolonged series of cycles, the existence of a true periodical element is apparent.

In the Madras observations the successive values of $\frac{S}{n}$ (obtained by combining one after another the observations of one cycle of 11 years, of two such cycles, of three, and so on, till the whole six are united) are shown below, contrasted with the corresponding calculated values of $\frac{E}{\sqrt{m}}$.

TABLE XII.

Number of cycles.	From observation, $\frac{S}{n}$.	Calculated, $\frac{E}{\sqrt{m}}$.
1 cycle	13·2	13·2
2 cycles	9·5	10·0
3 ,, ...	7·3	6·9
4 ,, ...	4·9	6·4
5 ,, ...	4·5	5·3
6 ,, ...	4·5 ?	5·1
12 ,, ...	3·4	3·8
18 ,, ...	3·2	3·0
24 ,, ...	2·7	2·7

The last cycle of the six is not complete, and the value of $\frac{S}{n}$ is therefore doubtful. As the observations only extend to six cycles, in order to test the process I have continued the calculation to twelve, eighteen, and twenty-four cycles, by combining with the real observations the three series formed at hazard by a chance redistribution of the original observations. This of course gives no support to any conclusions as to the *existence* of periodicity in the case of Madras; but it shows how, in the *absence* of periodicity, the theory is completely verified.

The Bombay, Calcutta, and Greenwich observations, similarly treated, exhibit similar results.

In strong contradistinction to the above results are those that follow on treating in a similar manner the barometric observations given in Tables VIII. and IX.; while these same observations, when their true periodicity is destroyed, as in Tables X. and XI., show results quite similar to those seen in Table XII.

The following Table shows these results, both with the periodicity as it really exists and after it has been destroyed:—

TABLE XIII.

Number of cycles.	Real periodicity.		Periodicity destroyed.	
	From observation, $\frac{S}{n}$.	Calculated, $\frac{E}{\sqrt{m}}$.	From observation, $\frac{S}{n}$.	Calculated, $\frac{E}{\sqrt{m}}$.
1 cycle	37·1	......	29·4	......
2 cycles	31·0	25·2	17·8	26·2
3 „	29·4	19·0	14·7	19·0
4 „	29·5	16·2	10·7	16·8
5 „	29·5	13·0	5·6	14·6
6 „	......	......	1·9	13·0

The fact that the figures in the last column but one are so much less than the calculated values of $\frac{E}{\sqrt{m}}$ evidently arises from the want of conformity of the numerical values of the quantities which are found in Table VIII. with the assumed law of the probable numerical distribution of errors on which the theoretical value $\frac{E}{\sqrt{m}}$ rests; but the mutual destruction of the non-periodical elements earlier than this theory would have required does not affect the general reasoning.

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"On the Calculation of the Trajectories of Shot." By W. D. Niven, M.A., F.R.A.S. Communicated by J. Clerk Maxwell, F.R.S., Professor of Experimental Physics in the University of Cambridge. Received March 24, 1876 *.

In the present state of our knowledge of the resistance of the air to shot, the problem of integrating the equations of motion of the shot and of plotting-out a representation of the curve described by it is peculiar, because, according to the best experiments we possess, the law of the retardation cannot be expressed by a single exact formula which is available for the solution. We are therefore compelled to give a solution adapted to Tables, the magnitudes of the retardation being set down in those Tables for velocities which are common in practice. The formulæ given by Hutton and by Didion, even if they were true, apply only to spherical shot; and though they are very simple formulæ, the solutions obtained by means of them are not satisfactory—first, by reason of their complexity, and next on account of the rough approximations which characterize the proofs.

Prof. Hélie, who gives an account of Didion's method in his 'Traité de Balistique,' says that it gives results which are not in accordance with fact. The fault may probably be laid in a great measure to the charge of the formula; for there can be no doubt that Mr. Bashforth's method of experimenting with his chronograph and screens gives more trustworthy and more extensive information than the ballistic pendulum experiments of Hutton and Didion; and Hutton's formula, as well as Didion's, agrees with Mr. Bashforth's Tables only for a limited range of velocities.

Mr. Bashforth himself makes no attempt to condense his Tables into concise formulæ. Accordingly, as we shall presently see, he adopts a solution which is capable of being employed in conjunction with his Tables. He divides the trajectory into small arcs, and finds for every arc the time and the horizontal and vertical distances from one end of the arc to the other. The entire trajectory may then be plotted out, and the whole time and range may be discovered, as well as the final inclination of the direction of motion. The amount of labour, however, in calculating all the quantities for a single component arc, even with the aid of copious tables, is so great that I was led to examine whether any thing could be done towards simplifying the solution and reducing the amount of calculation. It will appear in the sequel that rules of comparatively easy application can be employed, and that the tables necessary for their use are already existing or can be easily formed.

Meanwhile, without entering into details, it will be convenient to give a brief account of the drift and scope of what is attempted in this paper.

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* Read May 11, 1876. See vol. xxv. p. 18.

The work is arranged under three heads, which are called the First, the Second, and the Third Methods, intended to signify three distinct and different solutions of the problem of finding the motion of shot.

The First, which is the one adopted in Mr. Bashforth's treatise, is a solution on the assumption that the retardation due to air-resistance is μv^3, where μ is a constant. Now in the actual case μ is not constant; and therefore, in dealing with the equations of motion over any component part of the trajectory, a mean value of μ must be used.

In contrast to the first method, the third adopts the mean value of another quantity, viz. the inclination of the direction of motion. The same thing was done in General Didion's solution, in which it was necessary to employ the mean value of the cosine of the inclination. It will be found that in selecting this mean we really implicate more quantities than μ. But then there will be this advantage, that whereas there is no way of determining the mean of μ by the first method, and the greatest uncertainty prevails regarding it when comparatively large arcs are integrated over, according to the work now presented the nature of the mean is investigated. In fact the determination of the mean angle may be said to be the chief object and point in these investigations, because it will be seen that there is no difficulty in establishing any of the equations which will be used, and, excepting the labour, no difficulty in forming the Tables.

What is described as the Second Method, although a distinct solution when the retardation is formulated by μv^n, is to be regarded as a mere stepping-stone to the one which follows it. It is in the second method that all the quantities are expressed in terms of the mean inclination and the magnitude of that angle determined. The quantity μ is taken constant; and therefore, in the case when $n=3$, if we use the same mean value of μ as in the first method, we ought to get a solution much the same. Beyond this, the second method possesses no further practical value in the business now in hand, being entirely superseded in that respect. Its chief value and importance consist in the determination of the mean angle, because it is shown that the same value of that mean may be used in the third method.

This point being settled, it will be found that, according to the latter method, all the required quantities will depend on three integrals, two of which (the space- and time-integrals) have already been tabulated by Mr. Bashforth. The third, which may be called the velocity-integral, is now also tabulated for ogival-headed shot, and will be found further on.

§ 2. It seems convenient at the outset to define the symbols which will be employed throughout the work :—

v will denote the velocity of the shot at any point of the trajectory;

u, the horizontal component of v;

ϕ, the inclination of the direction of motion to the horizontal line in the plane of the trajectory (the deflection from the plane being neglected);

t, the time;
x, the horizontal distance from some fixed point;
y, the vertical distance.

The integrations will be performed over a component arc of the trajectory, and the three last quantities are measured from the beginning of the arc. The values of t, x, and y over the whole arc will be denoted by T, X, and Y. The values of u at the beginning and end of the arc will be denoted by p and q; those of ϕ by α and β. The acceleration due to gravity is denoted by g.

FIRST METHOD.

§ 3. The solution adopted by Mr. Bashforth is the famous one first given by John Bernoulli and published in 1721. It applies to any retardation formulated by μv^n. All the characteristic quantities are expressed in terms of ϕ, which may be accomplished briefly thus:—

The equations of motion in the horizontal direction and in the direction of the normal to the trajectory in the ascending branch are

$$\frac{du}{dt} = -\mu v^n \cos\phi, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$v\frac{d\phi}{dt} = -g\cos\phi. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Hence

$$\frac{du}{d\phi} = \frac{\mu}{g} v^{n+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$= \frac{\mu}{g} u^{n+1} \sec^{n+1}\phi.$$

Integrate this, recollecting that the initial values of u and ϕ are p and α, and get

$$\frac{1}{u^n} - \frac{1}{p^n} = \frac{\mu}{g}\int_\phi^\alpha n \sec^{n+1}\phi \, d\phi.$$

If the symbol P_ϕ denotes $\int_0^\phi n \sec^{n+1}\phi \, d\phi$, the equation will become

$$\frac{1}{u^n} - \frac{1}{p^n} = \frac{\mu}{g}(\mathrm{P}_\alpha - \mathrm{P}_\phi).$$

The horizontal velocity at the end of the arc is therefore given by

$$\frac{1}{q^n} = \frac{1}{p^n} + \frac{\mu}{g}(\mathrm{P}_\alpha - \mathrm{P}_\beta). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{A})$$

At the vertex of the trajectory the velocity u_0 is given by

$$\frac{1}{u_0^n}=\frac{1}{p^n}+\frac{\mu}{g}\mathrm{P}_\alpha\,;$$

$$\therefore\quad \frac{1}{u^n}=\frac{1}{u_0^n}\left\{1-\frac{\mu u_0^n}{g}\mathrm{P}_\phi\right\}.$$

If we put

$$\gamma=\frac{\mu u_0^n}{g}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \qquad (4)$$

we have

$$u=\frac{u_0}{\left(1-\gamma\mathrm{P}_\phi\right)^{\frac{1}{n}}}. \quad . \; . \; . \; . \; . \qquad (5)$$

From equation (2),

$$\mathrm{T}=\int_\beta^\alpha \frac{v}{g}\sec\phi\,d\phi$$

$$=\int_\beta^\alpha \frac{u}{g}\sec^2\phi\,d\phi\,;$$

by (5),

$$=\frac{u_0}{g}\int_\beta^\alpha \frac{\sec^2\phi\,d\phi}{\left(1-\gamma\mathrm{P}_\phi\right)^{\frac{1}{n}}}. \quad . \; . \; . \; . \; . \; . \; . \qquad (\mathrm{B})$$

Again,

$$\frac{dx}{dt}=u\,;$$

$$\therefore\quad x=\int u\,dt=-\int\frac{u^2}{g}\sec^2\phi\,d\phi\,;$$

$$\therefore\quad \mathrm{X}=\frac{u_0^2}{g}\int_\beta^\alpha\frac{\sec^2\phi}{\left(1-\gamma\mathrm{P}_\phi\right)^{\frac{2}{n}}}\,d\phi. \quad . \; . \; . \; . \; . \; . \qquad (\mathrm{C})$$

Similarly,

$$\mathrm{Y}=\frac{u_0^2}{g}\int_\beta^\alpha\frac{\tan\phi\sec^2\phi\,d\phi}{\left(1-\gamma\mathrm{P}_\phi\right)^{\frac{2}{n}}}. \quad . \; . \; . \; . \; . \; . \qquad (\mathrm{D})$$

§ 4. The law of retardation found by Mr. Bashforth, in his own symbols, is expressed by $2bv^3$, where

$$2b=\frac{d^2}{\mathrm{W}}\frac{\mathrm{K}}{(1000)^3},$$

d being the diameter of the cross section of the shot in inches, W its weight in pounds, and K a quantity which is a function of the velocity, and whose values are tabulated between certain values of the velocity for every ten feet. In integrating the equations of motion over any arc, the quantity K, and therefore $2b$, is taken constant, its mean value over the arc as near as it can be guessed at being used.

Taking this law, the above results, written in the order in which they would be used by a calculator, are as follows:—

$$\left(\frac{1000}{q}\right)^3-\left(\frac{1000}{p}\right)^3=\frac{d^2}{\mathrm{W}}\frac{\mathrm{K}}{g}(\mathrm{P}_\alpha-\mathrm{P}_\beta), \quad . \quad . \quad . \quad . \quad (a)$$

$$\left(\frac{1000}{u_0}\right)^3-\left(\frac{1000}{p}\right)^3=\frac{d^2}{\mathrm{W}}\frac{\mathrm{K}}{g}\mathrm{P}_\alpha, \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

$$\gamma=\frac{d^2}{\mathrm{W}}\frac{\mathrm{K}}{g}\left(\frac{u_0}{1000}\right)^3, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

$$\mathrm{T}=\frac{u_0}{g}\int_\beta^\alpha\frac{\sec^2\phi\, d\phi}{(1-\gamma\mathrm{P}_\phi)^{\frac{1}{3}}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

$$\mathrm{X}=\frac{u_0^2}{g}\int_\beta^\alpha\frac{\sec^2\phi\, d\phi}{(1-\gamma\mathrm{P}_\phi)^{\frac{2}{3}}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (e)$$

$$\mathrm{Y}=\frac{u_0^2}{g}\int_\beta^\alpha\frac{\tan\phi\sec^2\phi\, d\phi}{(1-\gamma\mathrm{P}_\phi)^{\frac{2}{3}}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (f)$$

The last three integrals Mr. Bashforth has given ample tables for, corresponding to different values of γ and between certain ranges of angle. If there is no table for the exact value of γ which results from equation (c), a method of interpolation must be employed. The integral P_ϕ is in this case $3\tan\phi+\tan^3\phi$, and its values are tabulated as well as those of its logarithm.

It will thus be seen that there are six distinct operations of some length, the first being the most serious, because there is some difficulty in getting K right. Supposing, however, that point to be settled—and I shall afterwards offer a few observations which will, I think, make the solution of (a) easier—the quantity γ must be found. It will be seen that (b) and (c) are mere stepping-stones to the time- and distance-integrals.

I shall now enter into an examination of the equations of motion, with the object of proving other formulæ, which, when we have once discovered the velocity at the end of the arc, will give the time and distances with two operations.

SECOND METHOD.

§ 5. I remark, first, that although Bernoulli's solution succeeds in expressing all the quantities in terms of integrals of ϕ, yet, owing to the difficulty and complexity of the integrals, it is practically valueless, except in two cases—first when $n=1$, and next when $n=3$. I doubt, moreover, whether, in the case $n=3$, it is the best solution when μ is not constant, and its mean value must therefore be taken over a small arc. The tables being such as they are, there can be no doubt that, if it were possible, the most convenient independent variable would be the velocity itself. On the other hand, all attempts at humouring the equations of motion so as to introduce the velocity as independent variable are of no avail. When the trajectory is very flat, it may be possible to get results which are not very objectionable; but no general solution is hereby attainable. The forms of the equations, however, suggest as a possibly good substitute the horizontal component of the velocity. Accordingly, taking this quantity as independent variable, I now proceed to find the distance-integrals by a method of approximation.

Since

$$\frac{dx}{dt}=u,$$

and

$$\frac{du}{dt}=-\mu v^n\cos\phi,$$

$$\therefore\quad \frac{dx}{du}=-\frac{1}{\mu}\cos^{n-1}\phi\,\frac{1}{u^{n-1}};$$

$$\therefore\quad \mathrm{X}=\frac{1}{\mu}\int_q^p\cos^{n-1}\phi\,\frac{du}{u^{n-1}}.$$

Similarly,

$$\mathrm{Y}=\frac{1}{u}\int_q^p\sin\phi\cos^{n-2}\phi\,\frac{du}{u^{n-1}}.$$

§ 6. Our business now is to substitute for ϕ its value in terms of u. To enable us to do this, put $\phi=\alpha-\psi$ and expand in powers of ψ: we have

$$\mathrm{P}_\alpha-\mathrm{P}_\phi=\frac{d\mathrm{P}}{d\alpha}\psi-\text{ etc.};$$

$$\therefore\quad \frac{1}{u^n}-\frac{1}{p^n}=\frac{\mu}{g}\left(n\sec^{n+1}\alpha\psi-\frac{n(n+1)}{2}\sec^{n+1}\alpha\tan\alpha\psi^2+\text{ etc.}\right). \tag{6}$$

Also

$$\left.\begin{aligned}\cos^{n-1}\phi=\cos^{n-1}\alpha+(n-1)\cos^{n-2}\alpha\sin\alpha\psi&\\ +\frac{n-1}{2}\{(n-2)\cos^{n-3}\alpha\sin^2\alpha-\cos^{n-1}\alpha\}\psi^2&\ .\\ +\text{etc.}\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .&\end{aligned}\right\}(7)$$

And

$$\left.\begin{aligned}\sin\phi\cos^{n-2}\phi=&\sin\alpha\cos^{n-2}\alpha+\{(n-2)\sin^2\alpha\cos^{n-3}\alpha-\cos^{n-1}\alpha\}\psi\\&+\{(n-2)(n-3)\sin^2\alpha-(3n-5)\cos^2\alpha\}\sin\alpha\cos^{n-4}\alpha\,\frac{\psi^2}{2}\\&+\text{etc.}\;\ldots\ldots\ldots\end{aligned}\right\}. \quad (8)$$

I now propose to neglect the squares and higher powers of ψ. The effect of this on the (c) integral will be that we have now to find X from

$$\frac{\cos^{n-1}\alpha}{\mu}\int_q^p\frac{du}{u^{n-1}}+\frac{(n-1)\cos^{n-2}\alpha\sin\alpha}{\mu}\int_q^p\psi\frac{du}{u^{n-1}}.$$

Call the two integrals in this expression Q and R. Then

$$Q=\frac{1}{n-2}\left(\frac{1}{q^{n-2}}-\frac{1}{p^{n-2}}\right);$$

and, taking account of equation (6),

$$\begin{aligned}R&=\frac{g}{\mu}\frac{\cos^{n+1}\alpha}{n}\int_q^p\left(\frac{1}{u^{2n-1}}-\frac{1}{p^n}\frac{1}{u^{n-1}}\right)du\\&=\frac{g}{\mu}\frac{\cos^{n+1}\alpha}{n}\left\{\frac{1}{2n-2}\left(\frac{1}{q^{2n-2}}-\frac{1}{p^{2n-2}}\right)-\frac{1}{n-2}\frac{1}{p^n}\left(\frac{1}{q^{n-2}}-\frac{1}{p^{n-2}}\right)\right\}.\end{aligned}$$

Let this be put equal to

$$Q(\alpha-\beta)f.$$

Then, since

$$\alpha-\beta=\frac{g}{\mu}\frac{\cos^{n+1}\alpha}{n}\left(\frac{1}{q^n}-\frac{1}{p^n}\right),$$

we see that

$$f=\frac{\frac{1}{2n-2}\left(\frac{1}{q^{2n-2}}-\frac{1}{p^{2n-2}}\right)-\frac{1}{n-2}\frac{1}{p^n}\left(\frac{1}{q^{n-2}}-\frac{1}{p^{n-2}}\right)}{\frac{1}{n-2}\left(\frac{1}{q^{n-2}}-\frac{1}{p^{n-2}}\right)\left(\frac{1}{q^n}-\frac{1}{p^n}\right)}.$$

Now let $\frac{1}{q}-\frac{1}{p}=\lambda$, so that $p\lambda\left(=\frac{p}{q}-1\right)$ may be regarded as a fraction whose square may be neglected. We get

$$f=\frac{\frac{n}{2}(p\lambda)^2+\frac{3n^2-7n}{6}(p\lambda)^3+\frac{7n^3-36n^2+47n}{24}(p\lambda)^4+\ldots}{n(p\lambda)^2\left\{1+(n-2)p\lambda+\frac{7n^2-32n+37}{12}(p\lambda)^2+\ldots\right\}}, \quad . \; . \quad (9)$$

$$=\tfrac{1}{2}\frac{1+\frac{3n-7}{3}p\lambda}{1+(n-2)p\lambda}\text{ if we neglect }(p\lambda)^2;$$

$$\therefore\ f=\tfrac{1}{6}\frac{3q+(3n-7)(p-q)}{q+(n-2)(p-q)}$$

$$=\tfrac{1}{6}\frac{(3n-7)p-(3n-10)q}{(n-2)p-(n-3)q}.$$

The value of X is now seen to become

$$\frac{1}{\mu}\{\cos^{n-1}\alpha+(n-1)\cos^{n-2}\alpha\sin\alpha(\alpha-\beta)f\}Q=\frac{1}{\mu}\cos^{n-1}\bar{\phi}Q,$$

where

$$\bar{\phi}=\alpha-(\alpha-\beta)f$$

$$=\frac{\alpha+\beta}{2}+(\tfrac{1}{2}-f)(\alpha-\beta)$$

$$=\frac{\alpha+\beta}{2}+\frac{p-q}{(n-2)p-(n-3)q}\cdot\frac{\alpha-\beta}{6}.$$

It is obvious that the value of Y may be obtained in exactly the same way, and that its magnitude is

$$\frac{1}{\mu}\cos^{n-2}\bar{\phi}\sin\bar{\phi}Q.$$

§ 7. It is necessary now to enter into an examination of the magnitudes of the errors committed in neglecting squares of the quantity ψ, and to discuss how far (that is to say, for what size of arc) we may with safety employ the values of X and Y just obtained.

Before doing so, I shall briefly remark on the step that we took in neglecting $(p\lambda)^2$ in the value of f. On an examination of equation (9), it will be seen that the principal part of the error committed amounts to

$$-\frac{2n-5}{12}(p\lambda)^2.$$

The error, therefore, in the angle $\bar{\phi}$ is

$$\frac{2n-5}{12}\frac{(p-q)^2}{q^2}(\alpha-\beta).$$

The consequent errors in X and Y, if we put D for the number of degrees in $\alpha-\beta$, are given by

$$\frac{\delta X}{X}=-\tan\bar{\phi}\cdot\frac{(n-1)(2n-5)}{12}\left(\frac{p-q}{q}\right)^2\frac{\pi}{180}D,$$

$$\frac{\delta Y}{Y}=\text{a similar expression.}$$

For the sake of simplicity, and to fix our ideas, let us take the case of

$n=3$, and suppose that we are integrating over an arc of 5°, in which one fourth of the horizontal velocity is lost. Then

$$\frac{\delta X}{X} = -\tan\bar{\phi} \cdot \tfrac{1}{3100};$$

$$\frac{\delta Y}{Y} = \cot 2\bar{\phi}\, \tfrac{1}{1550} \text{ approximately.}$$

It will therefore be observed that the error committed is trifling. As $\bar{\phi}$ will be small over the greater part of any trajectory, this error will chiefly affect Y; but by good fortune the sign of the error is opposite to the error we shall presently find, and therefore helps to neutralize that error.

Returning to the integral for X, we are now to take account of the square of ψ. Let the right-hand side of equation (6) be put equal to

$$\frac{\mu}{g} n \sec^{n+1} a\psi'.$$

Then

$$\psi' = \psi - \frac{n+1}{2} \tan a\psi^2,$$

$$\therefore\ \psi = \psi' + \frac{n+1}{2} \tan a\psi'^2.$$

Substituting this value of ψ in the expansions of $\cos^{n-1}\phi$ and $\cos^{n-2}\phi \sin\varphi$, we get

$$\begin{aligned}\cos^{n-1} a + (n-1) &\cos^{n-2} a \sin a\psi' \\ &+ \frac{n-1}{2} \{2n \sin^2 a - 1\} \cos^{n-3} a\psi'^2 \\ &+ \text{etc.},\end{aligned}$$

and

$$\begin{aligned}\cos^{n-2} a \sin a &+ \{(n-2) \sin^2 a - \cos^2 a\} \cos^{n-3} a\psi \\ &+ \{2(n-1)(n-2) \sin^2 a - 4(n-1) \cos^2 a\} \cos^{n-4} a \sin a \frac{\psi'^2}{2} \\ &+ \text{etc.}\end{aligned}$$

When the expressions just found are substituted in the (C) and (D) integrals, it is obvious that we have to determine these two integrals—

$$R' = \int_q^p \psi' \frac{du}{u^{n-1}},$$

$$S' = \int_q^p \psi'^2 \frac{du}{u^{n-1}}.$$

Referring to the investigation of R in § 6, we see that the work for R′ will be the same, except that instead of $\alpha-\beta$ we should have to put an angle ψ' corresponding to it, such that

$$\psi'=\alpha-\beta-\frac{n+1}{2}\tan\alpha(\alpha-\beta)^2.$$

The error in X due to the difference between R′ and R is ∴

$$-\frac{n^2-1}{2\mu}\cos^{n-3}\alpha\sin^2\alpha(\alpha-\beta)^2Qf.$$

The integral S′ may be reduced in the same way as the integral R. The most important part of it will be found to be

$$Q\frac{(\alpha-\beta)^2}{3}.$$

The corresponding error in X is therefore

$$\frac{n-1}{2\mu}(2n\sin^2\alpha-1)\cos^{n-3}\alpha\frac{(\alpha-\beta)^2}{3}Q.$$

In the former of these two components of the error of X it will be sufficient for the purposes of an estimate to put $f=\frac{1}{2}$. In that case the sum of the two component errors (call it δX) amounts to

$$\frac{n-1}{2\mu}\left\{\frac{2n\sin^2\alpha-1}{3}-\frac{n+1}{2}\sin^2\alpha\right\}\cos^{n-3}\alpha(\alpha-\beta)^2Q$$

$$=\frac{n-1}{2\mu}\left\{\frac{(n-3)\sin^2\alpha-2}{6}\right\}\cos^{n-3}\alpha(\alpha-\beta)^2Q.$$

It may be shown in a similar way that the error in Y is given by

$$\delta Y=\frac{1}{\mu}\left\{\frac{(n-1)(n-3)\sin^2\alpha-5n+11}{12}\right\}\sin\alpha\cos^{n-4}\alpha(\alpha-\beta)^2Q.$$

A discussion of these expressions for any assigned value of n would determine for what magnitude of arc we might with safety employ the formulæ for X and Y. I shall confine myself to the case when $n=3$, and for the purposes of a ready estimate I shall take

$$X=\frac{1}{\mu}\cos^2\frac{\alpha+\beta}{2}Q,$$

$$Y=\frac{1}{\mu}\cos\frac{\alpha+\beta}{2}\sin\frac{\alpha+\beta}{2}Q.$$

It thus appears that when $n=3$,

$$\frac{\delta X}{X}=-\frac{2}{1+\cos(\alpha+\beta)}\frac{(\alpha-\beta)^2}{3},$$

and

$$\frac{\delta Y}{Y}=-\frac{2\tan\alpha}{\sin(\alpha+\beta)}\frac{(\alpha-\beta)^2}{3}.$$

Now suppose we were integrating over an arc of 5°, then $\frac{(\alpha-\beta)^2}{3}$ might, approximately, be put equal to $\frac{25}{3\times3281}=\frac{1}{393}$. Our results would therefore be less than $\frac{1}{300}$ in error. Moreover the error in Y, which is really the more important of the two, is less than this, as I have pointed out at the beginning of this article. If, therefore, the formulæ are otherwise serviceable, their inherent errors do not seem to be a great objection to their use.

§ 8. The formulæ for X and Y already found apply only to the ascending branch of the trajectory. A little consideration enables us to see that the same formulæ apply to the descending branch, provided the mean angle $\bar{\phi}$ is

$$\frac{\alpha+\beta}{2}-\frac{p-q}{(n-2)p-(n-3)q}\frac{\beta-\alpha}{6},$$

β being now greater than α.

§ 9. In the preceding articles we have neglected all consideration of the time-integral. It may, of course, be treated in the same way as the distance-integrals. I shall not, however, go into the general case, but shall merely state the result in the important case when $n=3$,

$$T=\frac{\cos^2\bar{\phi}'}{\mu}\int_q^p\frac{du}{u^3},$$

where $\bar{\phi}'$ is equal to

$$\frac{\alpha+\beta}{2}+\frac{p-q}{9p-3q}\frac{(\alpha-\beta)}{2}$$

in the ascending branch, and

$$\frac{\alpha+\beta}{2}-\frac{p-q}{9p-3q}\frac{(\beta-\alpha)}{2}$$

in the descending.

If the arc integrated over is small, $\bar{\phi}'$ is not very different from $\bar{\phi}$.

§ 10. I propose now to collect the results I have proved, stating them in the order and form in which they would be used in conjunction with Mr. Bashforth's tables for K.

Summary of Results when $n=3$, $\mu=\frac{d^2}{W}\frac{K}{1000^3}$.

$$\left(\frac{1000}{q}\right)^3-\left(\frac{1000}{p}\right)^3=\frac{d^2}{W}\frac{K}{g}(P_\alpha-P_\beta). \quad . \quad . \quad . \quad . \quad . \quad \text{(a)}$$

$$\left.\begin{aligned} 2\bar{\phi}&=\alpha+\beta+\frac{p-q}{p}\,\frac{\alpha-\beta}{3}\ \text{(ascending branch)}, \quad . \quad . \quad . \\ \text{or}\quad &\ \alpha+\beta-\frac{p-q}{p}\,\frac{\beta-\alpha}{3}\ \text{(descending branch)}, \quad . \quad . \\ 2\bar{\phi}'&=\alpha+\beta+\frac{(p-q)}{9p-3q}(\alpha-\beta)\ \text{(ascending branch)}, . \quad . \\ \text{or}\quad &\ \alpha+\beta-\frac{(p-q)}{9p-3q}(\beta-\alpha)\ \text{(descending branch)}; \quad . \end{aligned}\right\}\ \text{(b)}$$

$$X=\frac{W}{d^2}(1+\cos 2\bar{\phi})\frac{500{,}000}{K}\left(\frac{1000}{q}-\frac{1000}{p}\right), \quad . \quad . \quad . \quad . \quad \text{(c)}$$

$$Y=\frac{W}{d^2}\sin 2\bar{\phi}\ \frac{500{,}000}{K}\left(\frac{1000}{q}-\frac{1000}{p}\right), \quad . \quad . \quad . \quad . \quad . \quad \text{(d)}$$

$$T=\frac{W}{d^2}(1+\cos 2\bar{\phi}')\frac{250}{K}\left(\overline{\frac{1000}{q}}\Big|^2-\overline{\frac{1000}{p}}\Big|^2\right). \quad . \quad . \quad . \quad . \quad \text{(e)}$$

These formulæ might very easily be used if the calculator were furnished with tables of $\frac{1000}{N}$ and $\left(\frac{1000}{N}\right)^2$ where N ranges between the magnitudes of the velocity occurring in practice.

Remarks on the Equation giving the Fall of Velocity in an Arc.

§ 11. It will be observed that the two foregoing methods each open with the same equation (a). Now there is a serious difficulty in the use of that equation. Suppose, for example, we were to integrate over an arc of 1°: we should have to use the mean value of K between its values corresponding to the velocities at the beginning and end of the arc. But we do not know the latter of these velocities; it is the very thing we have to find. The first steps in our work must therefore be to guess at it. The practised calculator can, from his experience, make a very good estimate. Having made his estimate, he determines K. He uses this value of K in equation (a); and if he gets the velocity he guessed at, he concludes that he guessed rightly and that he has got the velocity at the end of the arc. If the equation (a) does not agree with him, he makes another guess; and so on, till he comes right. It seems to me, however, that this method of going to work, leaving out of account the loss of time, is open to objection in the point of accuracy. For, first, there is no method of determining on what principle the mean value of K is to be found—what manner of mean it is. Again, let us suppose for an instant that the velocity at the end of the arc, guessed at, and the value of K are in agreement; that is to say, let the equation

$$\left(\frac{1000}{v_\beta}\right)^3 \sec^3\beta - \left(\frac{1000}{v_\alpha}\right)^3 \sec^3\alpha = \frac{d^2}{W}\frac{K}{g}(P_\alpha - P_\beta)$$

hold for the values of v_β and K used by the calculator. It by no means follows that he has hit on the right v_β and K. For if he is dealing with a part of the tables in which $\frac{dK}{dv}$ happens to be nearly equal to

$$-\frac{3Wg}{d^2}\frac{\sec^3\beta}{P_\alpha - P_\beta}\frac{(1000)^3}{v^4},$$

it is obvious that there are ever so many pairs of values of v_β and K which will stand the test of satisfying the above equation. Now an examination of Mr. Bashforth's tables for ogival-headed shot shows that the value of K diminishes as v increases from 1200 feet upwards, so that $\frac{dK}{dv}$ is negative for a considerable range of values of v which are common in practice. It is not at all unlikely, therefore, that the value for $\frac{dK}{dv}$ just stated may often be very nearly true; in which case the process of guessing becomes extremely dangerous.

I shall in the next method give a plan for determining the velocity at the end of the arc, which seems to me simpler and more satisfactory than the one we have been now discussing. Meanwhile, in connexion with the present method, the following considerations are worthy of notice.

§ 12. Let us consider the fundamental equation

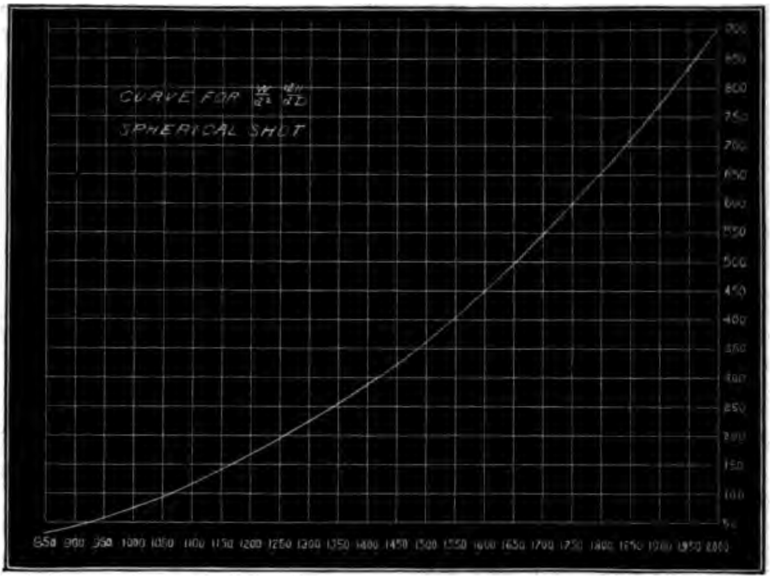
$$\frac{du}{d\phi} = \frac{d^2}{W}\frac{K}{g}\frac{v^4}{\overline{1000}|^3}.$$

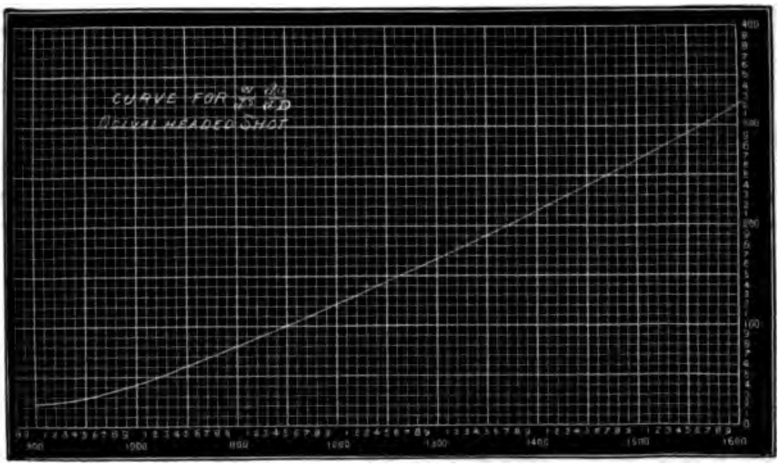
If $\frac{du}{d\phi}$ were uniform, it would be the fall of horizontal velocity in change of inclination equal to the unit of circular measure. As that angle is inconveniently large for discussions connected with trajectories of shot, let us take 1° for unit-angle, and let D be the number of degrees in the angle whose circular measure is ϕ. We then have

$$\frac{W}{d^2}\frac{du}{dD} = \frac{K}{g}\frac{\pi}{180} \times 1000 \times \left(\frac{v}{1000}\right).$$

The quantity on the right-hand side is the same for all shot of the same kind; and I propose to find its values for intervals of the values of v. It will be convenient to represent those values by the ordinates of a curve whose abscissæ are the corresponding velocities. Besides the immediate purpose now in view, the same curve will be useful in exhibiting the manner of change of the retardation; since if y be the ordinate corresponding to velocity v, the corresponding retardation will be

$$\frac{d^2}{W}\frac{180g}{\pi}\frac{y}{v}.$$





The primary use of these curves is that they enable us at a glance to make a rough estimate of the loss of velocity over an arc of one degree; and we have seen that such an estimate must first be made if we are to employ either of the foregoing methods.

It is possible to make the preliminary guess more near the truth by considering the approximate character of the curves. For example, the curve for ogival-headed shot is approximately a straight line, the tangent of whose inclination from 1700 down to 1250 is ·5; and from 1250 down to 1000, ·4; below 1000 it is unsafe to assign a value. We can easily prove for flat trajectories that the fall of velocity in one degree is either

$$\frac{W}{d^2}\left(\frac{du}{dD}\right)_0\left(\frac{1-e^{-\cdot 5\frac{d^2}{W}}}{\cdot 5}\right) \quad \text{or} \quad \frac{W}{d^2}\left(\frac{du}{dD}\right)_0\left(\frac{1-e^{-\cdot 4\frac{d^2}{W}}}{\cdot 4}\right)$$

between the values above assigned.

THIRD METHOD.

§ 13. Returning now to the fundamental equation

$$\frac{du}{d\phi}=\frac{Rv}{g},$$

where R is the retardation due to the resistance of the air, since R is some function of v let us put it equal to $f(v)$. Then, with our old notation still in use, we get by integration

$$g\int_q^p \frac{du}{vf(v)}=\alpha-\beta,$$

$$\therefore\ g\int_q^p \frac{du}{u\sec\phi f(u\sec\phi)}=\alpha-\beta.$$

Now instead of taking some mean value of the quantity K, as was done in the two previous methods, let us make the supposition that the quantity ϕ has its mean value—a supposition in this case by no means extravagant, since for the greater part of the trajectory $\sec\phi$ will vary very slowly. We then have, if D is the number of degrees in $\alpha-\beta$,

$$\frac{180g}{\pi}\int_q^p \frac{du}{u\sec\bar{\phi} f(u\sec\phi)}=D.$$

Now put $u=V\cos\bar{\phi}$. The equation becomes

$$\frac{180g}{\pi}\int_{q\sec\bar{\phi}}^{p\sec\bar{\phi}} \frac{dV}{Vf(V)}=D\sec\bar{\phi}.$$

With Mr. Bashforth's law substituted, this is

$$\frac{180g}{\pi}(1000)^3\int_{q\sec\bar{\phi}}^{p\sec\bar{\phi}}\frac{dV}{KV^4}=\frac{d^2}{W}D\sec\bar{\phi}. \quad . \quad . \quad . \quad . \quad (a')$$

Now the quantity

$$\frac{180g}{\pi}\,\overline{1000}|^3\int\frac{dV}{KV^4}$$

can be calculated for every 10 feet, by giving K its mean value over the 10 feet, beginning with 1700 for ogival-headed shot and with 2150 for spherical. Supposing this were done, and the results for all the components of 10 feet added together, the number opposite any velocity v would, for example, in the ogival-head-shot tables, be the value of

$$\frac{180g}{\pi}\,\overline{1000}|^3\int_{v}^{1700}\frac{dV}{KV^4}.$$

If we denote this quantity by V_v, the equation (a′) may be written

$$V_{q\sec\bar{\phi}}-V_{p\sec\bar{\phi}}=\frac{d^2}{W}D\sec\bar{\phi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a')$$

which may be regarded as an equation for the determination of q.

The distance-ordinates and time may be found in a similar manner. They are given by

$$\frac{d^2}{W}X=\cos\bar{\phi}\int_{q\sec\bar{\phi}}^{p\sec\bar{\phi}}\frac{\overline{1000}|^3dV}{KV^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b')$$

$$\frac{d^2}{W}Y=\sin\bar{\phi}\int_{q\sec\bar{\phi}}^{p\sec\bar{\phi}}\frac{\overline{1000}|^3dV}{KV^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c')$$

$$\frac{d^2}{W}T=\int_{q\sec\bar{\phi}}^{p\sec\bar{\phi}}\frac{\overline{1000}|^3dV}{KV^3}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d')$$

If we denote by S_v and T_v the two integrals

$$\int_{v}^{1700}\frac{\overline{1000}|^3dV}{KV^2}\quad\text{and}\quad\int_{v}^{1700}\frac{\overline{1000}|^3dV}{KV^3},$$

the equations (b′), (c′), and (d′) become

$$\frac{d^2}{W}X=\cos\bar{\phi}\,(S_{q\sec\bar{\phi}}-S_{p\sec\bar{\phi}}), \quad . \quad . \quad . \quad . \quad . \quad (b')$$

$$\frac{d^2}{W}\,Y=\sin\bar{\phi}\,(S_{q\sec\bar{\phi}}-S_{p\sec\bar{\phi}}), \quad . \quad . \quad . \quad . \quad . \qquad (c')$$

$$\frac{d^2}{W}\,T=T_{q\sec\bar{\phi}}-T_{p\sec\bar{\phi}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (d')$$

The quantities S_v and T_v have been tabulated by Mr. Bashforth for a considerable range of values of v, the upper limit being either 1700 or 2150, according as the shot is ogival-headed or spherical. The tables for the ogival-headed shot have recently been revised and carried to one place further in decimals. The quantities S_v and T_v may therefore be fortunately regarded as completely determined; and the only question will be regarding the mean angle $\bar{\phi}$. Now it is a remarkable circumstance that the value obtained for $\bar{\phi}$ in § 7, if q is not widely different from p, is very nearly the same for all values of n which are not far off from 3. But for limited portions of the trajectory the retardation may be considered as varying according to some simple power of the velocity, though that power is not the same from point to point, but still not far from 3. We may therefore take the value of $\bar{\phi}$ found in § 7 as applicable to the method now in hand. The adoption of this value of the mean angle, since $Y=X\tan\bar{\phi}$, is really equivalent to supposing the shot to move parallel to the chord; and the above proof shows what the limits of integration must be in order that the supposition may be made to approximate to the actual case. The most sensitive quantity in this method, especially near the vertex of the trajectory, is Y; and in finding it over an arc corresponding to a change of inclination as large as 5°, it is necessary to use the correct value obtained in § 7. As regards the other three quantities given by the integrals (a'), (b'), and (d'), it will not matter much if we take $\frac{\alpha+\beta}{2}$, at least for flat trajectories. If, however, the trajectory is not flat, and extreme accuracy is needful, it will be necessary to determine by (a') the quantity q twice over—first an approximate value of it, in order to get the mean angle $\bar{\phi}$, next its value with the mean angle used in the equation.

The following Table gives the value of V_v for ogival-headed shot as low as 900 feet per second. Below that value of the velocity Mr. Bashforth does not give tables for the resistance, and the magnitudes of the resistance for the lower velocities of ogival-headed shot have yet to be found.

Table of Values of $V_v = \frac{180g}{\pi}(1000)^3 \int_v^{1700} \frac{dV}{KV^4}$.

	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
90...	8·3380	8·2955	8·2529	8·2104	8·1678	8·1253	8·0827	8·0402	7·9976	7·9551
91...	7·9125	7·8714	7·8302	7·7890	7·7479	7·7066	7·6654	7·6242	7·5830	7·5418
92...	7·5006	7·4627	7·4248	7·3868	7·3489	7·3110	7·2730	7·2351	7·1972	7·1592
93...	7·1213	7·0851	7·0489	7·0127	6·9766	6·9404	6·9043	6·8681	6·8319	6·7958
94...	6·7596	6·7251	6·6906	6·6561	6·6216	6·5870	6·5525	6·5180	6·4835	6·4490
95...	6·4146	6·3819	6·3492	6·3166	6·2839	6·2513	6·2186	6·1860	6·1533	6·1207
96...	6·0880	6·0571	6·0263	5·9954	5·9646	5·9338	5·9030	5·8722	5·8413	5·8105
97...	5·7797	5·7507	5·7217	5·6927	5·6637	5·6347	5·6057	5·5767	5·5477	5·5187
98...	5·4898	5·4625	5·4352	5·4079	5·3806	5·3533	5·3260	5·2987	5·2715	5·2442
99...	5·2169	5·1914	5·1660	5·1405	5·1151	5·0896	5·0641	5·0387	5·0132	4·9878
100...	4·9623	4·9386	4·9149	4·8912	4·8675	4·8437	4·8200	4·7963	4·7726	4·7489
101...	4·7252	4·7032	4·6812	4·6591	4·6371	4·6151	4·5931	4·5711	4·5490	4·5270
102...	4·5050	4·4847	4·4643	4·4440	4·4236	4·4033	4·3830	4·3626	4·3423	4·3219
103...	4·3016	4·2829	4·2642	4·2456	4·2269	4·2082	4·1895	4·1708	4·1522	4·1335
104...	4·1148	4·0977	4·0806	4·0635	4·0464	4·0293	4·0122	3·9951	3·9780	3·9609
105...	3·9439	3·9282	3·9125	3·8968	3·8811	3·8655	3·8498	3·8341	3·8184	3·8027
106...	3·7871	3·7726	3·7581	3·7436	3·7292	3·7147	3·7002	3·6857	3·6712	3·6567
107...	3·6422	3·6287	3·6151	3·6016	3·5881	3·5746	3·5610	3·5475	3·5340	3·5204
108...	3·5069	3·4941	3·4814	3·4686	3·4558	3·4431	3·4303	3·4175	3·4047	3·3920
109...	3·3792	3·3670	3·3549	3·3427	3·3306	3·3184	3·3062	3·2941	3·2819	3·2698
110...	3·2576	3·2460	3·2344	3·2228	3·2111	3·1995	3·1879	3·1762	3·1646	3·1529
111...	3·1413	3·1301	3·1190	3·1078	3·0966	3·0855	3·0743	3·0631	3·0519	3·0408
112...	3·0296	3·0189	3·0081	2·9974	2·9867	2·9760	2·9652	2·9545	2·9438	2·9330
113...	2·9223	2·9120	2·9017	2·8913	2·8810	2·8707	2·8604	2·8501	2·8397	2·8294
114...	2·8191	2·8092	2·7992	2·7893	2·7794	2·7695	2·7595	2·7496	2·7397	2·7297
115...	2·7198	2·7102	2·7007	2·6916	2·6816	2·6721	2·6625	2·6530	2·6435	2·6339
116...	2·6244	2·6152	2·6059	2·5967	2·5874	2·5782	2·5689	2·5597	2·5504	2·5412
117...	2·5319	2·5230	2·5141	2·5052	2·4963	2·4874	2·4785	2·4696	2·4607	2·4518
118...	2·4429	2·4343	2·4257	2·4171	2·4085	2·3999	2·3913	2·3827	2·3741	2·3655
119...	2·3570	2·3486	2·3403	2·3320	2·3237	2·3154	2·3071	2·2988	2·2905	2·2822
120...	2·2739	2·2659	2·2578	2·2498	2·2418	2·2338	2·2257	2·2177	2·2097	2·2016
121...	2·1936	2·1858	2·1781	2·1703	2·1625	2·1548	2·1470	2·1392	2·1314	2·1237
122...	2·1159	2·1084	2·1008	2·0933	2·0858	2·0783	2·0707	2·0632	2·0557	2·0481
123...	2·0406	2·0333	2·0260	2·0188	2·0115	2·0042	1·9969	1·9896	1·9824	1·9751
124...	1·9678	1·9607	1·9537	1·9466	1·9396	1·9325	1·9254	1·9184	1·9113	1·9043
125...	1·8972	1·8903	1·8834	1·8766	1·8697	1·8629	1·8561	1·8492	1·8424	1·8355
126...	1·8287	1·8221	1·8154	1·8088	1·8022	1·7956	1·7889	1·7823	1·7757	1·7690
127...	1·7625	1·7560	1·7496	1·7432	1·7367	1·7303	1·7239	1·7174	1·7110	1·7045
128...	1·6981	1·6919	1·6856	1·6794	1·6731	1·6669	1·6606	1·6544	1·6481	1·6419
129...	1·6356	1·6295	1·6234	1·6174	1·6113	1·6052	1·5991	1·5931	1·5870	1·5810
130...	1·5749	1·5689	1·5630	1·5571	1·5512	1·5453	1·5394	1·5334	1·5275	1·5216
131...	1·5157	1·5099	1·5042	1·4984	1·4927	1·4870	1·4813	1·4756	1·4698	1·4641
132...	1·4584	1·4528	1·4472	1·4417	1·4361	1·4305	1·4249	1·4193	1·4138	1·4082
133...	1·4026	1·3972	1·3918	1·3863	1·3809	1·3755	1·3701	1·3646	1·3592	1·3538
134...	1·3484	1·3431	1·3378	1·3325	1·3272	1·3220	1·3167	1·3114	1·3061	1·3008
135...	1·2955	1·2904	1·2852	1·2801	1·2749	1·2698	1·2646	1·2595	1·2543	1·2492
136...	1·2440	1·2390	1·2340	1·2289	1·2239	1·2189	1·2139	1·2089	1·2038	1·1988
137...	1·1938	1·1889	1·1840	1·1791	1·1742	1·1693	1·1644	1·1595	1·1546	1·1497
138...	1·1448	1·1400	1·1352	1·1305	1·1257	1·1209	1·1161	1·1113	1·1066	1·1018
139...	1·0970	1·0923	1·0876	1·0829	1·0783	1·0736	1·0689	1·0643	1·0596	1·0549
140...	1·0503	1·0457	1·0412	1·0366	1·0321	1·0275	1·0229	1·0184	1·0138	1·0093
141...	1·0047	1·0002	·9958	·9913	·9869	·9824	·9779	·9735	·9690	·9646
142...	·9601	·9557	·9514	·9470	·9427	·9383	·9339	·9296	·9252	·9209
143...	·9165	·9122	·9079	·9036	·8994	·8951	·8908	·8866	·8823	·8780
144...	·8737	·8695	·8653	·8612	·8570	·8528	·8486	·8444	·8402	·8361
145...	·8319	·8278	·8237	·8196	·8155	·8114	·8073	·8032	·7991	·7950

TABLE (*continued*).

	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.
146...	·7910	·7870	·7830	·7790	·7750	·7710	·7669	·7629	·7589	·7549
147...	·7509	·7470	·7430	·7391	·7352	·7312	·7273	·7234	·7194	·7155
148...	·7115	·7077	·7038	·6999	·6961	·6922	·6884	·6845	·6806	·6768
149...	·6729	·6691	·6654	·6616	·6578	·6540	·6502	·6465	·6427	·6389
150...	·6351	·6314	·6277	·6239	·6202	·6165	·6128	·6091	·6053	·6016
151...	·5979	·5943	·5906	·5870	·5833	·5797	·5760	·5724	·5687	·5651
152...	·5614	·5578	·5542	·5506	·5470	·5435	·5399	·5363	·5327	·5291
153...	·5255	·5220	·5185	·5149	·5114	·5079	·5044	·5009	·4973	·4938
154...	·4903	·4868	·4834	·4799	·4765	·4730	·4695	·4661	·4626	·4592
155...	·4557	·4523	·4489	·4455	·4421	·4387	·4352	·4318	·4284	·4250
156...	·4216	·4183	·4149	·4116	·4082	·4049	·4015	·3982	·3948	·3915
157...	·3881	·3848	·4815	·3782	·3749	·3716	·3684	·3651	·3618	·3585
158...	·3552	·3520	·3487	·3455	·3422	·3390	·3357	·3325	·3292	·3260
159...	·3227	·3195	·3163	·3132	·3100	·3068	·3036	·3004	·2973	·2941
160...	·2909	·2877	·2846	·2814	·2783	·2751	·2719	·2688	·2656	·2625
161...	·2593	·2562	·2531	·2500	·2469	·2439	·2408	·2377	·2346	·2315
162...	·2284	·2254	·2223	·2193	·2162	·2132	·2102	·2071	·2041	·2010
163...	·1980	·1950	·1920	·1890	·1860	·1831	·1801	·1771	·1741	·1711
164...	·1681	·1652	·1622	·1593	·1563	·1534	·1505	·1475	·1446	·1416
165...	·1387	·1358	·1329	·1300	·1271	·1243	·1214	·1185	·1156	·1127
166...	·1098	·1070	·1042	·1013	·0985	·0957	·0929	·0900	·0872	·0844
167...	·0816	·0788	·0760	·0732	·0705	·0677	·0649	·0621	·0594	·0566
168...	·0538	·0511	·0484	·0456	·0429	·0402	·0375	·0347	·0320	·0293
169...	·0266	·0239	·0213	·0186	·0159	·0133	·0106	·0080	·0053	·0027

An example of the use of these Tables was given in the abstract which was printed in the 'Proceedings' of the Society, vol. xxv. p. 18.

POSTSCRIPT.

Professor J. Couch Adams, to whom this paper was shown before publication, has obtained a solution of the equations employed in the second method, which is of a more complete and satisfactory character than the one given above. The results he has arrived at are contained in the following note, which he has kindly allowed me to subjoin:—

"Employing the notation of the paper, and supposing the resistance to vary as the *n*th power of the velocity, the horizontal velocity q is given by the equation

$$\frac{1}{q^n}-\frac{1}{p^n}=\frac{\mu n}{g}\left(\sec\frac{\alpha+\beta}{2}\right)^{n+1}(\alpha-\beta)\left\{1+\tfrac{1}{24}(n+1)\left[(n+2)\left(\sec\frac{\alpha+\beta}{2}\right)^2-(n+1)\right](\alpha-\right.$$

where $\alpha-\beta$ is expressed in the circular measure.

"The inclination $\bar{\phi}$ of the chord AB is given by

$$\bar{\phi}=\frac{\alpha+\beta}{2}+\tfrac{1}{3}\,\frac{p-q}{p+q}(\alpha-\beta)+\tfrac{1}{4}\left(\tan\frac{\alpha+\beta}{2}\right)(\alpha-\beta)^2,$$

where $\alpha-\beta$ is supposed to be expressed as before in the circular measure. If $\alpha-\beta$ be expressed in minutes, the last term must be multiplied by sin 1′.

"The value of the mean angle $\bar{\phi}'$ to be employed in finding the time-integral is

$$\bar{\phi}'=\frac{\alpha+\beta}{2}+\tfrac{1}{8}\frac{p-q}{p+q}(\alpha-\beta)+\tfrac{1}{4}\left(\tan\frac{\alpha+\beta}{2}\right)(\alpha-\beta)^2,$$

where the last term is the same as that in the above value of $\bar{\phi}$, but the second term is only one half of its amount in the former case.

"It will be seen that the above expressions for $\bar{\phi}$ and $\bar{\phi}'$ are independent of the value of n.

"Also, if

$$Q=1-\tfrac{1}{24}(n-1)\left[(n-2)\left(\sec\frac{\alpha+\beta}{2}\right)-(n-3)\right](\alpha-\beta)^2,$$

where $\alpha-\beta$ is expressed as before in the circular measure, the values of the coordinates X, Y, and of the time T are given by

$$X=\frac{1}{\mu(n-2)}Q(\cos\bar{\phi})^{n-1}\left(\frac{1}{q^{n-2}}-\frac{1}{p^{n-2}}\right),$$

$$Y=\frac{1}{\mu(n-2)}Q(\cos\bar{\phi})^{n-2}\sin\bar{\phi}\left(\frac{1}{q^{n-2}}-\frac{1}{p^{n-2}}\right)=X(\tan\bar{\phi}),$$

$$T=\frac{1}{\mu(n-1)}Q(\cos\bar{\phi}')^{n-1}\left(\frac{1}{q^{n-1}}-\frac{1}{p^{n-1}}\right).$$

"It may be remarked that if α and β, as well as p and q, be interchanged, the values of $\bar{\phi}$, $\bar{\phi}'$, and Q will remain unaltered, and the values of X, Y, and T will merely change their signs, as it is evident should be the case.

"The above values of $\bar{\phi}$, $\bar{\phi}'$, and Q are true to the third order of small quantities inclusive, and the values of X, Y, and T are true to the fourth order, considering $\frac{p-q}{p+q}$ and $\alpha-\beta$ to be small quantities of the first order."

June 7, 1877.

The Annual Meeting for the election of Fellows was held this day.

Sir JOSEPH HOOKER, C.B., K.C.S.I., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Dr. E. H. Greenhow and Mr. T. Sopwith were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following candidates were declared duly elected into the Society :—

Prof. James Dewar, M.A.
Sir Joseph Fayrer, M.D., K.C.S.I.
Rev. Norman Macleod Ferrers, M.A.
Thomas Richard Fraser, M.D.
Brian Haughton Hodgson, F.L.S.
John W. Judd, F.G.S.
William Carmichael M'Intosh, M.D.
Robert M'Lachlan, F.L.S.
Prof. John William Mallet, Ph.D.
Henry B. Medlicott, M.A.
Henry Nottidge Moseley, M.A.
Prof. Osborne Reynolds, M.A.
William Roberts, M.D.
Prof. James Thomson, LL.D.
Prof. William Turner, M.B.

Thanks were given to the Scrutators.

June 14, 1877.

Sir JOSEPH HOOKER, C.B., K.C.S.I., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

His Majesty Pedro II., Emperor of Brazil, Sir Henry Barkley, Prof. James Dewar, Sir Joseph Fayrer, Rev. Norman Macleod Ferrers, Prof. John Wesley Judd, Mr. Robert M'Lachlan, Prof. Osborne Reynolds, Dr. William Roberts, Prof. James Thomson, and Prof. William Turner were admitted into the Society.

The following Papers were read :—

I. "On the Minute Structure and Relationships of the Lymphatics of the Mammalian Skin, and on the Ultimate Distribution of Nerves to the Epidermis and Subepidermic Lymphatics." By GEORGE HOGGAN, M.B., and FRANCES ELIZABETH HOGGAN, M.D. Communicated by Dr. WILLIAM FARR, F.R.S. Received May 11, 1877.

(Abstract.)

The authors state that, by means of certain modifications in known methods of histological research, a full description of which they offer, they have been enabled to show the minute structure and relationships of the lymphatics of the skin in mammals.

For the purpose of anatomical description, they divide these lymphatics into three categories, named, from their position, the subhypodermic, the dermic, and the subepidermic. Only the first and third can be described as layers; the second consists of horizontal and vertical sets of vessels, extending through the whole thickness of the dermis, and connecting the other two distinct layers together.

All the lymphatics of the hypodermis, and most of those of the dermis, are valved efferent vessels, without any collecting channels that would entitle them to claim any absorbing function in these portions of the skin, through which they merely pass.

The subepidermic lymphatics are narrow parallel collecting channels, destitute of valves, lying, as their name implies, immediately under the epidermic cells in young animals, although separated from them, as adult life is reached, by bundles of gelatinous tissue. These are the only radicles of the lymphatics of the skin.

Upon the subepidermic lymphatics they find a rich plexus, formed by multipolar nerve-cells and non-medullated nerve-fibres, the distribution of which to the epidermis has been made evident by the same process. As no acknowledged contractile elements enter into the walls of these lymphatics, the function of the nerves found upon them cannot be affirmed by the authors.

Neither sweat-glands, sebaceous glands, hair-muscles, fat-cells, nor nerve bundles possess any lymphatics, and the papillæ in the human skin are equally destitute of them. Functionally, the lymphatics of the skin are to be considered as forming two classes, the valved efferent vessels with independent walls, formed only of crenated endothelium cells, and the valveless collecting channels of the subepidermis, lined by those crenated cells.

Upon the facts accumulated in this and their former paper the authors are led entirely to reject the theory of vasa serosa or radicles of the lymphatics, formed by chains of connective-tissue cells or the cavities in which they lie. In the human skin especially these cells of the connective tissue are numerous and in intimate relationship with the superficial

blood-vessels, but prominently absent from the collecting lymphatic channels lying alongside of these vessels, thus supporting the hypothesis they formerly emitted, that these cells were merely links in a nutritive chain, not radicles of the lymphatics, even when, as in tendon, the cornea, &c., they are connected with the lymphatics. The paper is illustrated by about a dozen and a half of camera-lucida drawings of microscopical specimens in their possession.

II. "Refractive Indices of Glass." By J. Hopkinson, D.Sc., M.A. Communicated by Professor G. G. Stokes, Sec. R.S. Received May 25, 1877.

Most of the following determinations were made two years ago. They were not published at once, because the results showed more variation than was expected. They are now made known for two reasons. First, most of the glasses examined are articles of commerce, and can be readily obtained by any person experimenting upon the physical properties of glass; these glasses only vary within narrow limits, and their variations may be approximately allowed for by a knowledge of their density. Second, most of the prisms having three angles from each of which determinations were made, the probable error of the mean is very small, and any error of the nature of a blunder is certainly detected.

The form in which to present these results was a matter of much consideration. A curve giving the refractive indices directly is unsuitable, for the errors of observation are less than the errors of curve-drawing would be. The theory of dispersion is not in a position to furnish a satisfactory rational formula. The most frequently used empirical formula is $\mu=a+b\frac{1}{\lambda^2}+c\frac{1}{\lambda^4}+\ldots$, where μ is the wave-length of the ray to which μ refers. But to bring this within errors of observation it is necessary to include $\frac{1}{\lambda^6}$, which appears to be almost as important a term as $\frac{1}{\lambda^4}$. There are two points of importance in the selection of an empirical form: first, it must accurately represent the facts with the use of the fewest arbitrary parameters; second, it must be practically convenient for the purposes for which the results are useful.

In the present case the most convenient form is

$$\mu-1=a\{1+bx(1+cx)\},$$

where x is a numerical name for the definite ray of which μ is the refractive index. In the present paper line F, being intermediate between the strongest luminous and chemical rays, is taken as zero. Four glasses, Hard Crown, Soft Crown, Light Flint, and Dense Flint, are selected on account of the good accord of the results, and the mean of their refractive indices $\bar{\mu}$ is ascertained for each ray: this is taken as a standard scale in which $x=\bar{\mu}-\bar{\mu}_F$.

If f_0 be the focal distance of a compound lens for line F, f_0', f_0'', &c. of the component lenses, then

$$\frac{1}{f}=\Sigma\frac{1}{f_0^{(r)}}+\Sigma\frac{b^{(r)}}{f_0^{(r)}}\,.\,x+\Sigma\frac{b^{(r)}c^{(r)}}{f_0^{(r)}}\,.\,x^2,$$

f being the focal length for the ray denoted by x.

If there be two lenses in the combination

$$\frac{1}{f}=\frac{1}{f_0}+\frac{1}{f_0}\,\frac{b'b''}{b''-b'}\,(c'-c'')x^2,$$

since the effect of changing the ray to be denoted by zero does not sensibly change the value of the coefficient $\frac{b'b''}{b''-b'}\,(c'-c'')$, this may be taken as a measure of the irrationality of the combination.

Let there be three glasses (1), (2), (3); no combination free from secondary dispersion and of finite focal length can be made with these glasses if

$$\begin{vmatrix} 1, & 1, & 1 \\ b', & b'', & b''' \\ b'c', & b''c'', & b'''c''' \end{vmatrix}=0.$$

Again, if the secondary chromatic aberration of (2) (1) is the same as that of (3) (1), then that of (2) (3) has also the same value, and the three glasses satisfy the above condition.

Prof. Stokes has expressed the character of glasses in the following manner:—Let a prism of small angle i be perfectly achromatized by two prisms of standard glasses with angles i', i'' taken algebraically as regards sign, then

$$ai+a'i'+a''i''=\text{deviation of any ray},$$
$$abi+a'b'i'+a''b''i''=0,$$
$$abci+a'b'c'i'+a''b''c''i''=0;$$

hence

$$\frac{i'}{i''}=\frac{c''-c}{c-c'}\,\frac{a''b''}{a'b'}.$$

If $c=c''$ this ratio is zero, but if $c=c'$ it is infinite; let $\frac{i'}{i''}=\tan\phi$, then the angle ϕ may be taken with a and b as a complete specification of the optical properties of the glass. Prof. Stokes's method has a great advantage in the close correspondence between the values of i, i', i'' and the powers of the component lenses of a perfectly achromatic object-glass, and also in the rapidity with which a determination can be made. The method adopted in this paper is convenient in the fact that a single standard glass is alone required.

The determinations were made with a spectrometer supplied to Messrs. Chance Bros. & Co. by Mr. Howard Grubb. The telescope and collimator are 2 inches aperture; the circle is 15 inches diameter, is graduated to 10′, and reads by two verniers to 10″.

The lines of the spectrum observed were generally A, B, C, D, E, b, F, (G), G, h, H_1. D is the more refrangible of the pair of sodium lines, b

is the most refrangible of the group of magnesium lines, (G) is the hydrogen line near G.

The method of smoothing the results by the aid of each other has been, first, to calculate a, b, and c from the mean values of μ for the lines B, F, H_1; second, to calculate values of μ from the formula obtained; third, to plot on paper the differences between μ observed and μ calculated, and to draw a free-hand curve among the points, and then inversely to take μ for each line from the curve.

It appeared desirable to express the standard values of $\bar{\mu}$, which are the means of those for Hard Crown, Soft Crown, Light Flint, and Dense Flint, in terms of $\frac{1}{\lambda^2}$. In the following Table column

I. gives λ, the wave-length in 10^{-4} centims.;

II. the values of $\frac{1}{\lambda^2}$;

III. the standard values of $\bar{\mu}$;

IV. the values of $\bar{\mu}$ calculated from

$$\mu=a+b\frac{1}{\lambda^2}+c\frac{1}{\lambda^4}, \text{ where}$$

$$a=1·539718,$$
$$b=0·0056349,$$
$$c=0·0001186;$$

V. the differences of III. and IV.;

VI. the values of $\bar{\mu}$ from the extended formula

$$\mu=a+b\frac{1}{\lambda^2}+c\frac{1}{\lambda^4}+d\frac{1}{\lambda^6}, \text{ where}$$

$$a=1·538414,$$
$$b=0·0067669,$$
$$c=-0·0001734,$$
$$d=0·000023;$$

VII. The differences of III. and VI.

	I. λ.	II. $\frac{1}{\lambda^2}$.	III. μ.	IV.	V.	VI.	VII.
B.	·68668	2·12076	1·552201	1·552201	0·000000	1·552203	− 2
C.	·65618	2·32249	1·553491	1·553444	+0·000047	1·553481	+10
D.	·58890	2·88348	1·557030	1·556951	+0·000079	1·557033	− 3
E.	·52690	3·60200	1·561612	1·561553	+0·000059	1·561613	− 1
b.	·51667	3·74605	1·562530	1·562490	+0·000040	1·562538	− 8
F.	·48606	4·23272	1·565692	1·565692	0·000000	1·565693	− 1
G.	·43072	5·39026	1·573459	1·573536	−0·000077	1·573457	+ 2
h.	·41012	5·94536	1·577356	1·577409	−0·000053	1·577349	+ 7
H_1.	*·39680*	6·35121	1·580287	1·580287	0·000000	1·580289	− 2

It is interesting to remark that the curve representing μ in terms of $\frac{1}{\lambda^2}$ has a point of inflexion between C and D. An examination of the deviations from calculation for several glasses shows that probably all glasses exhibit a similar point of inflexion, the flints lower in the spectrum or in the ultra red, and the crowns nearer to the middle of the visible spectrum. This fact may be of importance in the theory of dispersion when a detailed theory becomes possible; at least it is important as showing how unsafe it would be to calculate μ for very long waves or ultra-violet waves from any formula of three terms.

The following Tables of results need little or no further explanation; the first line gives the refractive indices finally obtained and regarded as most probable, the second line gives the values of μ from the formula of three terms

$$\mu - 1 = a\{1 + bx(1 + cx)\},$$

and the last gives the mean of the actual observations.

Hard Crown.—Two specimens of this glass were examined.

α. A prism from the three angles of which determinations were obtained, density 2·48575.

$a = 0{\cdot}523145$. $b = 1{\cdot}3077$. $c = -2{\cdot}33$.

	A.	B.	C.	D.	E.	*b*.	F.	(G.)	G.	*h*.	H_1.
Most probable value of μ	......	1·513625	1·514568	1·517114	1·520331	1·520967	1·523139	1·527994	1·528353	1·530902	1·532792
μ from formula............	......	1·513624	1·514560	1·517099	1·520327	1·520966	1·523145	1·528003	1·528362	1·530906	1·532789
Mean of observed values	1·511755	1·513624	1·514571	1·517116	1·520324	1·520962	1·523145	1·527996	1·528348	1·530904	1·532789

β. A prism with two angles of about 45°, density=2·48664.

The first line gives the mean observed indices, and the second the differences from the most probable values of α.

	A.	B.	C.	D.	E.	*b*.
Mean of observed values	1·572296	1·514156	1·515105	1·517623	1·520860	1·521474
	(0·000541)	0·000531	0·000537	0·000509	0·000529	0·000507

	F.	(G.)	G.	*h*.	H_1.
Mean of observed values	1·523657	1·528533	1·528856	1·531439	1·533318
	0·000518	0·000539	0·000503	0·000537	0·000526

Soft Crown.—The prism has three angles from which the mean is taken, density=2·55035.

$a=0·5209904$. $b=1·4034$. $c=-1·58$.

	A.	B.	C.	D.	E.	*b*.
Most probable value of μ	......	1·510916	1·511904	1·514591	1·518010	1·518686
μ from formula............	......	1·510918	1·511900	1·514574	1·517991	1·518670
Mean of observed values	1·508956	1·510918	1·511910	1·514580	1·518017	1·518678

	F.	(G.)	G.	*h*.	H_1.
Most probable value of μ	1·520996	1·526207	1·526595	1·529359	1·531416
μ from formula............	1·520994	1·526215	1·526603	1·529364	1·531418
Mean of observed values	1·520994	1·526208	1·526592	1·529360	1·531415

Titano-silicic Crown.—This glass was made on the suggestion of Professor Stokes in the hope of obtaining a glass of good quality which should be perfectly achromatic with a flint. The determinations were made on two angles; density=2·55255.

$a=0·550466$. $b=1·5044$. $c=-0·93$.

	B.	C.	D.	E.	*b.*	F.
Most probable value of μ	1·539155	1·540255	1·543249	1·547088	1·547852	1·550471
μ from formula............	1·539152	1·540249	1·543236	1·547074	1·547839	1·550466
Mean of observed values	1·539157	1·540253	1·543248	1·547088	1·547852	1·550471

	(G.)	G.	*h.*	H_1.		
Most probable value of μ	1·556386	1·556830	1·559999	1·562392		
μ from formula............	1·556407	1·556852	1·560015	1·562388		
Mean of observed values	1·556394	1·556825	1·559998	1·562392		

Extra Light Flint Glass.—Determinations made from three angles, density=2·86636.

$a=0·549123$. $b=1·7064$. $c=-0·198$.

	A.	B.	C.	D.	E.	*b.*
Most probable value of μ	......	1·536450	1·537673	1·541011	1·545306	1·546166
μ from formula..	......	1·536448	1·537663	1·540993	1·545296	1·546158
Mean of observed values	1·534067	1·536450	1·537682	1·541022	1·545295	1·546169

	F.	(G.)	G.	*h.*	H_1.	
Most probable value of μ	1·549121	1·555863	1·556372	1·560010	1·562760	
μ from formula............	1·549123	1·555881	1·556390	1·560026	1·562760	
Mean of observed values	1·549125	1·555870	1·556375	1·559992	1·562760	

Light Flint Glass.—Determinations made from three angles, density=3·20609.

$a=0·583887$. $b=1·9605$. $c=+0·53$.

	B.	C.	D.	E.	*b.*	F.
Most probable value of μ	1·568558	1·570011	1·574015	1·579223	1·580271	1·583886
μ from formula............	1·568553	1·570009	1·574017	1·579226	1·580274	1·583887
Mean of observed values	1·568558	1·570007	1·574013	1·579227	1·580273	1·583881

	(G.)	G.	*h.*	H_1.		
Most probable value of μ	1·592190	1·592824	1·597332	1·600727		
μ from formula............	1·592185	1·592815	1·597322	1·600724		
Mean of observed values	1·592184	1·592825	1·597332	1·600717		

Dense Flint.—Determinations from three angles, density=3·65865.

$a=0·634744$. $b=2·2694$. $e=1·48$.

	B.	C.	D.	E.	*b.*	F.
Most probable value of μ	1·615701	1·617484	1·622414	1·628895	1·630204	1·634748
μ from formula.....	1·615700	1·617488	1·622427	1·628904	1·630210	1·634744
Mean of observations ...	1·615704	1·617477	1·622411	1·628882	1·630208	1·634748

	(G.)	G.	*h.*	H_1.		
Most probable value of μ	1·645267	1·646068	1·651840	1·656219		
μ from formula............	1·645258	1·646060	1·651837	1·656222		
Mean of observations ...	1·645268	1·646071	1·651830	1·656229		

Extra Dense Flint.—Determinations from only one angle, density=3·88947.

$a=0{\cdot}664226.\quad b=2{\cdot}4446.\quad c=1{\cdot}87.$

	A.	B.	C.	D.	E.	b.
Most probable value of μ and μ from formula ...	......	1·642874	1·644866	1·650388	1·657653	1·659122
Observed	1·639143	1·642894	1·644871	1·650374	1·657631	1·659108

	F.	(G.)	G.	h.	H_1.
Most probable value of μ and μ from formula	1·664228	1·676111	1·677019	1·683577	1·688569
Observed	1·664246	1·676090	1·677020	1·683575	1·688590

Double Extra Dense Flint.—Determinations from two angles, density=4·42162.

$a=0{\cdot}727237.\quad b=2{\cdot}7690.\quad c=2{\cdot}70.$

	A.	B.	C.	D.	E.	b.
Most probable value of μ and μ from formula	......	1·701060	1·703478	1·710201	1·719114	1·720924
Mean of observations ...	1·696531	1·701080	1·703485	1·710224	1·719081	1·720908

	F.	(G.)	G.	h.	H_1.
Most probable value of μ and μ from formula	1·727237	1·742063	1·743204	1·751464	1·757785
Mean of observations ...	1·727257	1·742058	1·743210	1·751485	

The following Table gives the value of $\frac{bb'}{b'-b}(c-c')$ for each glass when combined with the standard. It serves to show how little there is to choose between the glasses ordinarily used.

Hard Crown.	Soft Crown.	Titanic Crown.	Extra Light Flint.	Light Flint.	Dense Flint.	Extra Dense Flint.	Double Extra Dense Flint.
−11·7	−10·7	−9·4	−9·9	−9·4	−11·8	−11·9	−13·2

III. "Electrostatic Capacity of Glass." By J. Hopkinson, D.Sc., M.A. Communicated by Prof. Sir William Thomson, F.R.S. Received May 17, 1877.

(Abstract.)

The instruments used in the experiments were—a guard-ring condenser the distance between the plates of which could be varied and measured; a sliding condenser which could be adjusted so that its capacity was equal to the former; the quadrant electrometer used as an electroscope; a battery of 72 Daniell's elements.

The method of experiment was to connect the middle of the battery with the lower plate of the guarding condenser and with the case of the electrometer, to charge one condenser from one pole of the battery, the other from the opposite pole, to insulate both, add their charges, and then connect them to the quadrant of the electrometer. The condensers are equal when there is no deflection of the image. Two experiments are needed in each case—1st, the glass plate is introduced into the guard-ring condenser, and the sliding condenser is made equal to it; 2nd, the glass plate is removed, and the guard-ring condenser is made equal to the sliding condenser. The two readings of the screw which measures the distance of the plates of the former give the specific inductive capacity of the glass.

Four samples of flint glass were successfully examined with the following results:—

	Density.	Electrostatic capacity.	Ratio.	Index of Refraction for line D.
Light flint	3·2	6·85	2·14	1·574
Double extra dense flint	4·5	10·1	2·25	1·710
Dense flint	3·66	7·4	2·02	1·622
Very light flint	2·87	6·57	2·29	1·541

Two samples of the first of these glasses were examined, taken from different meltings, and made at different times; they gave the same result. It will be remarked that these determinations do not bear out Prof. Maxwell's conclusion that the square of the refractive index for long waves is equal to the product of the electrostatic capacity and the magnetic permeability. The quotient electrostatic capacity ÷ density for these glasses and also for solid paraffin lies between 2·0 and 2·3.

IV. "On the Difference of Potential produced by the Contact of different Substances."

By R. B. Clifton, F.R.S., Professor of Experimental Philosophy in the University of Oxford.

Received May 22, 1877.

In order fully to determine the origin of the difference of electric potential which is exhibited at the terminals of a voltaic element, and on which the electromotive force of the element depends, it is necessary to investigate the difference of potential produced at each contact of the different substances contained in the element.

This difference of potential was approximately determined by Volta in certain cases of contact of two metals, and the investigation of the electric condition of metals in contact has since been more fully studied by Kohlrausch and others.

The difference of potential arising when a metal and a liquid are brought in contact was pointed out by Volta, and has since been investigated by Becquerel, Péclet, Pfaff, Buff, Kohlrausch, and others; but the results obtained have in most cases merely shown that a difference of potential exists, and indicated the sign of this difference.

The methods of observation adopted by Becquerel, Péclet, and Pfaff do not indeed deal with the simple case of the contact of a given metal and a given liquid, but introduce more than one active contact; and the method of Buff, which Kohlrausch has also employed, is objectionable from the fact that, in addition to a metal-liquid contact a metal-glass or metal-lac contact has been introduced; and this latter contact is, I believe, far from inoperative, producing, indeed, a very considerable difference of potential.

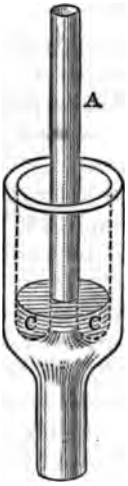
In endeavouring to study this subject I have suppressed the solid dielectric and used a condenser having its opposed surfaces of metal and liquid separated by a plate of air, and have been careful to introduce only a single operative metal-liquid contact.

Although circumstances have prevented me from making much progress with quantitative measurements—and, indeed, the experiments have indicated that a change must be introduced into the apparatus employed before satisfactory measures of the difference of potential in certain important cases can be obtained, this difference being much less than previous investigations had led me to expect—yet I venture to lay before the Society a short account of some of the results obtained, and I hope at some future time to be able to communicate a more complete investigation with reliable quantitative determinations.

For the insulation of the apparatus, on which so much depends, I have employed a modification of the form of insulator suggested by Sir W. Thomson, which seems to me more convenient than the form in common use, while it equals, if it does not surpass, the latter in efficiency.

Fig. 1 represents one of these insulators, which consists of a glass rod A, projecting from a glass cup B, to the bottom of which it is fused, so that the whole forms one piece of glass. Into the cup B a small quantity of strong sulphuric acid (C) is introduced, which appears effectually to dry the inside of the cup and a portion of the glass rod. In many cases, during the time required for an observation, I failed to detect any loss of charge, although six of these supports are used in the apparatus. Even without sulphuric acid in the cup, this form of stand seems to insulate fairly well; for, even in very damp weather, the loss from six such stands rarely exceeded one per cent. of the charge in a minute.

Fig. 1.



The electrometer employed was Sir W. Thomson's quadrant electrometer, in the simple form constructed by the late Mr. Becker, which seems to me to be more sensitive than the more complex instrument, and to give equally reliable results, when it is required only to *compare* differences of potential. With this instrument and a condenser one thousandth part of the difference of potential produced by the contact of zinc and copper is very distinctly perceptible.

The condenser was furnished with horizontal plates, which could be separately levelled, and the upper plate could be moved normally to its surface, and adjusted with any required interval between the plates. In observations of the difference of potential between a metal and a liquid, the latter was contained in a glass vessel placed upon the lower plate, and connected with this plate by a strip or wire of the same metal as that covering the lower face of the upper plate.

A form of key was used by which the condenser-plates can be connected by a metallic circuit, while the four quadrants of the electrometer are maintained at the same potential, and the plates of the condenser can then be connected with the respective terminals of the electrometer, the whole being well insulated. By the same key the alternate pairs of quadrants, in connexion with the respective plates of the condenser, can be charged to a definite difference of potential by a voltaic cell, and can then, while still in connexion with the condenser, be insulated without the least risk of introducing an unknown potential by touching the metallic portions of the apparatus by the hand.

With this apparatus the following experiments were performed.

The upper condenser-plate of copper was connected by an insulated copper wire with one terminal of the electrometer, the other terminal being connected by an insulated copper wire with the lower condenser-plate, which was again connected with a copper wire dipping in distilled water, contained in a vessel standing on the lower plate.

The two insulated wires connecting the condenser and electrometer were now brought into metallic connexion by the key, thus putting all four quadrants of the electrometer in connexion, and at the same time uniting the copper condenser-plate with the wire dipping in the water. The copper plate and the water thus become charged, the difference of potential being that due to the contact of copper and water.

The upper (copper) plate is next brought near the surface of the water, which acts as the lower plate of the condenser, the interval between the surfaces being from 0·1 millim. to 0·2 millim.

The metallic connexion between the connecting wires is now suppressed, and the upper condenser-plate quickly raised; the needle of the electrometer is at once deflected, and in the direction which indicates that the upper plate is *positive* with respect to the lower.

It appears, then, that when copper and distilled water are in contact they assume a difference of potential, copper being *positive* to distilled water.

Excepting Professor W. G. Hankel[*], all previous observers, with whose results I am acquainted, agree in asserting that copper is *negative* to water; but I have repeated the above experiment a great number of times under varied conditions, and always with the same result, so that I feel confident that copper is really *positive* to water.

When the vessel on the lower plate contained a saturated solution of copper sulphate, and similar operations were performed to those above described, the result was that copper and a saturated solution of copper sulphate assume in contact a difference of potential, copper being *positive* to the solution of copper sulphate.

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[*] Abhandl. d. königl. sächs. Gesellschaft der Wissenschaften, vol. xi. p. 605.

My experiments gave no indication of the change of sign of the potential of copper with respect to that of water, which, according to Professor Hankel, takes place during the first few minutes of contact. It is possible, however, that this change may have taken place, although the copper wire had not been immersed more than two or three minutes when the difference of potential was observed.

At the time my experiments were performed I had not seen Professor Hankel's paper.

The difference of potential in this case appears to be about $\frac{3}{10}$ of the difference of potential of copper and water in contact; but I do not yet feel in a position to make any very definite statements with respect to the relative magnitudes of the differences of potential of metals and liquids in contact.

When the liquid employed was commercial sulphuric acid diluted with twenty times its volume of distilled water, the difference of potential observed was very small, so small, indeed, that copper and this dilute acid in contact may be regarded as practically at the same potential; a very small difference of potential, however, exists in this case, and copper is *negative* to this dilute acid.

Copper and a strong aqueous solution of caustic potash assume in contact a small difference of potential, larger, however, than that observed in the last case, and copper is *negative* to the solution of caustic potash.

There is a very marked difference of potential between copper and an aqueous solution of potassium sulphide (liver of sulphur) in contact, copper being *negative* to this solution.

Between copper and a strong aqueous solution of potassium cyanide in contact there is a very considerable difference of potential, copper being *negative* to the solution. The difference of potential in this case is comparable with the difference observed when zinc and copper are in contact.

Similar experiments were performed with the upper condenser-plate of well-cleaned zinc, the liquid being connected with the lower plate by a strip of zinc. When the liquid is distilled water there is a marked difference of potential, zinc being *positive* to water; and the magnitude of this difference of potential appears to be, very approximately at least, the same as that observed in the case of copper and water.

With zinc and dilute sulphuric acid the difference of potential is extremely small; but zinc is slightly *negative* to the dilute acid (1 volume of sulphuric acid in 20 volumes of distilled water).

In experimenting with zinc the greatest care is necessary in cleaning the surface of the zinc condenser-plate, as zinc oxide is strongly negative to zinc; when the plate is very slightly tarnished, the sign of the difference of potential exhibited by the condenser, in the case of water, is reversed, the oxidized zinc being *negative* to distilled water.

When the upper condenser-plate was of iron, and distilled water acted as the lower plate, there was a small but distinct difference of potential, iron being *negative* to water.

The experiments above described seem to indicate that zinc and copper are about equally positive to water, and that consequently zinc and copper dipping in water are nearly at the same potential.

This conclusion has been also arrived at by Sir W. Thomson as the result of an experiment described by Professor F. Jenkin in his 'Text Book of Electricity and Magnetism;' but this experiment seems to leave something to be desired on the score of sensitiveness, and I was anxious to put the conclusion to a somewhat severer test. With this object the following experiment was performed:—

The condenser was furnished with carefully cleaned plates of zinc and copper, which were connected as before by insulated copper wires with the respective terminals of the electrometer. Into a vessel containing distilled water two insulated plates of zinc and copper respectively (both carefully cleaned) were plunged and connected with the key above mentioned by copper wires. By means of this key the zinc plate in water was connected with the zinc condenser-plate, and the copper plate in water with the copper condenser-plate, the two condenser-plates being separated by an interval certainly not more than 0·2 millim. The needle of the electrometer was at once deflected, and in a few minutes became stationary in a position showing a considerable deflection due to the difference of potential of the terminals of the zinc-water-copper element, while the zinc and copper plates of the condenser are necessarily at the same potentials as the zinc and copper plates in the water.

The key was now opened, so that the condenser-plates and electrometer, still in connexion as before, became insulated, and no change in the deflection of the electrometer-needle was perceptible.

The condenser-plates were now quickly separated, but the electrometer-needle remained absolutely undisturbed. It appears, then, that the zinc and copper plates in water are so nearly at the same potential, that the apparatus employed fails to show the difference between their potentials. Considering that if the zinc and copper condenser-plates are connected by a wire when at the same distance apart, and are then insulated and separated in the same way, the needle is so strongly deflected as to be turned completely round, I feel justified in stating that clean zinc and copper plates when first plunged in distilled water, if not absolutely at the same potential, certainly do not differ in potential by the thousandth part of the difference of potential produced by the contact of zinc and copper.

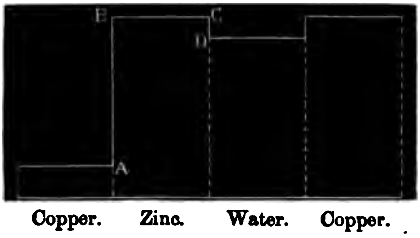
The electromotive force, then, of a voltaic element composed of zinc and copper plates dipping in distilled water, and connected by a copper wire, is at first, for all practical purposes, due entirely to the difference of potential produced by the contact of zinc and copper, the water having no perceptible effect.

This was assumed by Volta, on less conclusive evidence, and forms the basis of his reasoning with respect to the action of the pile devised by him.

I cannot, however, agree with Professor Jenkin that the water is also at the same potential as the zinc and copper plates immersed in it. The above-mentioned experiments seem to me to show that the water is negative to both plates, but to the same extent.

If the potentials of the different substances in the voltaic element just mentioned be represented by lines drawn perpendicular to an axis along which the substances are distributed in the order in which they occur in the element, the zinc being furnished with a copper terminal, the distribution of potential, when the circuit is not completed, is represented by fig. 2.

Fig. 2.

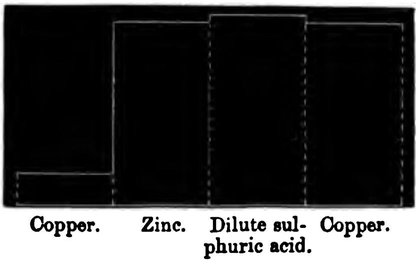


As I have not yet obtained sufficiently trustworthy measures of the difference of potential between zinc or copper and liquids, this figure and those which follow do not represent *quantitative* relations; the zinc-water difference of potential C D may possibly be represented as too large, compared with the zinc-copper difference of potential A B. The figures are merely intended to represent the *distribution* of the potentials.

Similar experiments were performed with zinc and copper plates dipping in dilute sulphuric acid (1 volume of acid in 20 volumes of distilled water), and with the same result, viz. that the zinc and copper plates are, as far as can be observed, at the same potential; and in this case, as the experiments previously described show, the liquid is very nearly at the same potential as the immersed metals.

It is possible that with more dilute sulphuric acid Professor Jenkin's statement will be found to be absolutely true, the zinc, copper, and dilute acid in which they are immersed being all at the same potential; but with the dilute acid used the distribution of potential in this voltaic element is represented, in the same manner as before, by fig. 3.

Fig. 3.



The series of metals and liquids which I have examined is small; but, so far as it extends, the phenomenon of two metals being at the same potential when immersed in a liquid is peculiar to zinc and copper, and only takes place in the case of these metals when the liquid is either water or dilute sulphuric acid, and when the metals have been recently placed in it.

For instance, when zinc and copper are immersed in a solution of caustic potash, there is a small but distinct difference of potential between the zinc and copper, copper being positive to zinc. Again, iron and copper in distilled water exhibit a marked difference of potential, the copper plate being positive to the iron plate; this is, of course, a necessary consequence of the fact, previously stated, that copper is positive to water, while iron is negative to water, and it is confirmed by the experiments with a copper-water-iron element.

Similar observations were made with zinc and iron in distilled water, with the result that zinc in water is positive to iron in water.

Connected with the experiment by which I have attempted to show that well-cleaned zinc and copper in water are at the same potential, there are some points which seem worthy of mention. When the condenser-plate of zinc is thoroughly cleaned, but the zinc plate in the water is not cleaned with the same care, the copper plate at first appears to be slightly *negative* to the zinc plate; but after a short time (the immersion of the plates being continued) this difference of potential disappears, and the two plates are, as before mentioned, exactly at the same potential. If the plates continue immersed a small difference of potential again appears between them; but now the copper appears to be *positive* to the zinc, and this difference of potential slowly increases until it attains, after some hours, a considerable magnitude.

A possible explanation of these changes seems to be that at first the zinc oxide has not been entirely removed, and, being strongly negative to the zinc, it causes the copper to appear negative to the zinc; but as the zinc becomes acted upon by the water*, its surface becomes, partially at least, coated with hydrogen, which is positive to zinc, and hence the copper plate, assuming the same potential, or nearly so, as the altered zinc surface, becomes positive to the zinc plate. As the time of immersion is prolonged the coating of hydrogen on the zinc becomes more complete, and the apparent positive potential of the copper with respect to the zinc increases, the zinc plate becoming less and less operative in the water, and serving more and more as a mere support for the hydrogen.

With amalgamated zinc the coating of hydrogen is formed much more evenly and rapidly; and thus, in spite of the presence on the zinc plate of the mercury, which is considerably *negative* to copper, an amalgamated zinc plate and a copper plate in water, or in dilute sulphuric acid, exhibit, when first immersed, a larger difference of potential than zinc and copper under the same circumstances. The result of my experiments up to the present has, however, led me to conclude that the final difference of potential between amalgamated zinc and copper in water, or dilute sulphuric acid, is very nearly, if not exactly, the same as that exhibited by zinc and copper under the same conditions, if the immersion of the plates is sufficiently prolonged.

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* The zinc used in these experiments was commercial, not pure zinc.

This rise of the difference of potential between the plates of a voltaic element while the circuit is open seems to me to be due to exactly the same cause as the so-called polarization of the plates when the circuit is closed, but operating in the opposite direction.

The natural result of this change in the difference of potential of the immersed plates is, that, in a cell composed of zinc and copper plates immersed in water, the zinc being furnished with a copper terminal, the difference of potential exhibited by the terminals should increase with the length of time the plates have been immersed, the circuit of course never having been completed. In order to verify this I have endeavoured to compare, by the electrometer, the difference of potential of the terminals of such a cell with the difference of potential of the terminals of a Daniell's cell. Several determinations gave nearly the same results; and the following numbers show the magnitude of the change which takes place:—

D represents the difference of potential of a Daniell's cell, in which the amalgamated zinc plate is immersed in a liquid composed of 1 part by weight of pure sulphuric acid and 4 parts by weight of distilled water. According to Mr. Latimer Clark, D = 1·079 volt.

The difference of potential of the copper terminals of a zinc-water-copper cell is,

immediately after the immersion of the plates,

0·760 D, or 0·820 volt;

after the plates have been immersed 1·5 hour,

0·821 D, or 0·886 volt;

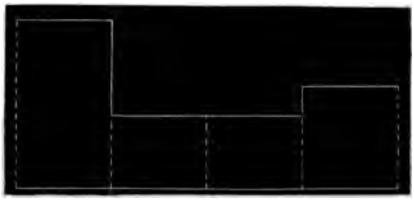
after the plates have been immersed 3 hours,

0·838 D, or 0·905 volt.

The experiments with the condenser above mentioned, by which the corresponding change in the potentials of the plates themselves was observed, would indicate that the difference of potential of zinc and copper in contact (which I will represent, as usual, by Zn | Cu) is between 0·760 D and 0·821 D, and nearer to the former. This indication is justified by the result of direct experiment, as described in a later part of this paper.

From the observations previously described as to the difference of potential of copper with respect to water and with respect to a solution of copper sulphate, it follows that when copper plates are immersed respectively in water and in a saturated aqueous solution of copper sulphate, the two liquids being separated by a porous partition, if no difference of potential arise between the liquids in contact, the copper plate in water will be *positive* to the copper plate in the copper-sulphate solution. The distribution of potential will be indicated by fig. 4.

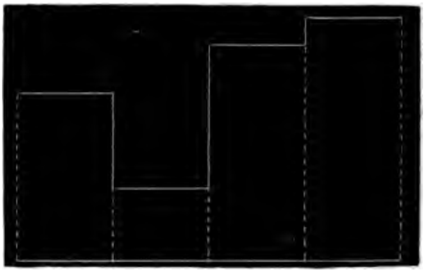
Fig. 4.



Experiment, however, shows that the copper plate in water is distinctly *negative* to the copper plate in the copper-sulphate solution. Hence it follows that there is a considerable difference of potential between these two liquids in contact, the copper-sulphate solution being positive to distilled water.

The real distribution of potential in this case is therefore represented in fig. 5; as before, these figures are not drawn to scale, but only represent qualitatively the distribution of potential.

Fig. 5.

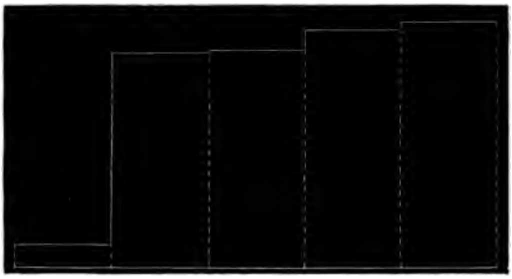


Similar experiments indicate (though, in the absence of perfectly reliable measures, perhaps not so conclusively) that a saturated solution of copper sulphate is positive to dilute sulphuric acid and also to a solution of potassium cyanide when the first liquid is in contact with either of the two latter.

Indeed the experiments I have performed lead me to conclude that the 11 to 13 per cent., by which the difference of potential exhibited by the copper terminals of a Daniell's cell exceeds the difference of potential, shown by the copper terminals of a cell composed of amalgamated zinc and copper in dilute sulphuric acid is mainly due to the contact of the dilute sulphuric acid and the solution of copper sulphate in the Daniell's cell. The contact between the copper and the copper-sulphate solution appears to add but little, and the contact of the zinc and dilute acid next to nothing, to the difference of potential; at all events in the case where the acid solution is weak.

In such a Daniell's cell the distribution of potential appears to be that indicated in fig. 6.

Fig. 6.



The very large difference of potential between copper and a solution of potassium cyanide in contact, to which attention has been called at the commencement of this paper, coupled with the fact that this difference is of the opposite sign to that exhibited by copper and water, or copper and a solution of copper sulphate in contact, shows that a voltaic cell of considerable power may be constructed containing only one metal and two liquids. Such cells have long been known; for instance, Becquerel's cell, composed of platinum in nitric acid, and platinum in a solution of caustic potash; but the only cell of this kind which I have seen described, in which copper is the only metal present, is that devised by the late Emperor Napoleon.

A cell composed of copper in water and copper in a solution of potassium cyanide (1 part by weight of cyanide to 5 parts by weight of distilled water) shows a difference of potential between its terminals equal to 0·923 D, or 0·996 volt.

This cell has, of course, a very large internal resistance; but this resistance may be greatly diminished by substituting a saturated solution of copper sulphate for the water; and although this substitution causes a loss of potential at the junction of the copper and liquid, yet it is much more than compensated by the gain at the junction of the two liquids.

Thus a cell composed of copper in a saturated solution of copper sulphate, and copper in a solution of potassium cyanide (1 part by weight of the cyanide to 5 parts by weight of distilled water), exhibits at its terminals a difference of potential equal to 1·102 D, or 1·189 volt.

In order to test the constancy of this last-mentioned cell, it was constructed in the form usually adopted for Grove's battery: the inner porous vessel contained 60 cub. centims. of the potassium-cyanide solution, and in it was inserted a plate of copper; the outer vessel contained a solution of copper sulphate maintained in a state of saturation by the presence of crystals of the salt, and the copper plate in this vessel was bent round the flat porous vessel so as nearly to touch it on both sides. The terminals were now connected by short wires with a tangent galvanometer, and the current allowed to pass uninterruptedly, with the following results:—

	Deflection of the needle.
	°
At the time the circuit was closed	34
After 6 minutes	58
„ 15 „	73
„ 22 „	75
„ 30 „	75
„ 45 „	68·5
„ 62 „	46
„ 75 „	28
„ 90 „	25
„ 105 „	23·5
„ 120 „	21·5

It appears, therefore, that though, for a voltaic element containing only one metal, this cell has a considerable electromotive force, it is far from constant in its action.

In endeavouring to compare the differences of potential arising in pairs of metals in contact, I have employed the method of Kohlrausch (Wiedemann's 'Galvanismus,' 1861, vol. i. p. 28), in which, by the aid of a condenser having its plates of two metals, the difference of potential of these metals, due to their contact, is compared with the difference of potential exhibited by the terminals of a constant voltaic element.

For the purpose of comparison I used a Clark's standard cell (Phil. Trans. vol. clxiv. p. 1), relying upon the reported constancy of the difference of potential of its terminals; and although I find that this difference is very far from constant, yet the rate of change is slow, and for experiments performed at intervals of only a few days it may be considered constant within the limits of the experimental errors to which my present apparatus seems liable.

I will denote by C the difference of potential of the terminals of this cell at the time the following measures were made:—

With condenser-plates of iron and copper carefully cleaned I obtained

$$\begin{aligned} \mathrm{Fe} \mid \mathrm{Cu} &= 0{\cdot}0774\ \mathrm{C.} \\ &\ 0{\cdot}0717\ \mathrm{C.} \\ \hline &\ 0{\cdot}0745\ \mathrm{C.}\ \text{(Mean.)} \end{aligned}$$

With condenser-plates of zinc and copper carefully cleaned immediately before commencing the series of measurements the following results were obtained:—

$$\begin{aligned} \mathrm{Zn} \mid \mathrm{Cu} &= 0{\cdot}6180\ \mathrm{C.} \\ &\ 0{\cdot}6238\ \mathrm{C.} \\ &\ 0{\cdot}6172\ \mathrm{C.} \\ &\ 0{\cdot}6204\ \mathrm{C.} \\ \hline &\ 0{\cdot}6198\ \mathrm{C.}\ \text{(Mean.)} \end{aligned}$$

In these experiments the circumstances were altered as much as possible, the condenser-plates were adjusted at different intervals, and the plates were in some cases separated slowly, and in others as rapidly as possible; yet the greatest amount of variation in the measures obtained scarcely exceeds one per cent.

These two measurements lead, by Volta's law, to

$$\mathrm{Zn} \mid \mathrm{Fe} = 0{\cdot}5453\ \mathrm{C.}$$

Direct experiments with condenser-plates of zinc and iron gave the following results:—

$$\begin{aligned} \mathrm{Zn} \mid \mathrm{Fe} &= 0{\cdot}5228\ \mathrm{C.} \\ &\ 0{\cdot}5329\ \mathrm{C.} \\ &\ 0{\cdot}5198\ \mathrm{C.} \\ \hline &\ 0{\cdot}5251\ \mathrm{C.}\ \text{(Mean.)} \end{aligned}$$

Unfortunately in these experiments the plates were not cleaned immediately before the measurements were made, and the zinc plate was very slightly oxidized, so that this series must give a value for Zn | Fe below the true value. For this reason more confidence is to be placed in the result deduced from the experiments with Zn and Cu and with Fe and Cu than in that obtained by the last series.

When mercury acted as the lower plate of the condenser and the upper plate was of iron, I obtained

$$\mathrm{Fe} \mid \mathrm{Hg} = 0{\cdot}23144\ \mathrm{C.}$$

The above-mentioned results lead, then, to the following:—

$$\begin{aligned} \mathrm{Fe} \mid \mathrm{Cu} &= 0{\cdot}120\ .\ \mathrm{Zn} \mid \mathrm{Cu.} \\ \mathrm{Zn} \mid \mathrm{Fe} &= 0{\cdot}880\ .\ \mathrm{Zn} \mid \mathrm{Cu.} \\ \mathrm{Fe} \mid \mathrm{Hg} &= 0{\cdot}373\ .\ \mathrm{Zn} \mid \mathrm{Cu.} \\ \mathrm{Zn} \mid \mathrm{Hg} &= 1{\cdot}253\ .\ \mathrm{Zn} \mid \mathrm{Cu.} \\ \mathrm{Cu} \mid \mathrm{Hg} &= 0{\cdot}253\ .\ \mathrm{Zn} \mid \mathrm{Cu.} \end{aligned}$$

Comparison of the difference of potential of zinc and copper in contact with that of the terminals of the standard Daniell's cell before mentioned gave

$$\text{Zn} \mid \text{Cu} = 0{\cdot}7892 \text{ D};$$

or, assuming Mr. Latimer Clark's measurement of D, viz. 1·079 volt,

$$\text{Zn} \mid \text{Cu} = 0{\cdot}8516 \text{ volt.}$$

This value shows that, on the occasion of the previous measures,

$$\text{C} = 1{\cdot}273 \text{ D} = 1{\cdot}374 \text{ volt.}$$

By using this value of Zn | Cu in the preceding results, the values contained in the following Table are obtained :—

	D.	Volt.
Zn | Fe	0·694	0·749
Fe | Cu	0·095	0·102
Cu | Hg	0·200	0·216
Zn | Cu	0·789	0·852
Fe | Hg	0·295	0·318
Zn | Hg	0·989	1·067

The experiments from which the above-mentioned ratios of Zn | Cu to the difference of potential of the terminals of Clark's standard cell are deduced were performed on January 5 and January 8, 1877. A series of similar experiments had been previously made on December 15 and December 16, 1876, which led to the following results, C′ representing the difference of potential of the terminals of Clark's cell at that time :—

$$\begin{aligned} \text{Zn} \mid \text{Cu} = {} & 0{\cdot}5863 \text{ C}'. \\ & 0{\cdot}5842 \text{ C}'. \\ & 0{\cdot}5912 \text{ C}'. \\ & 0{\cdot}5890 \text{ C}'. \\ \hline & 0{\cdot}5877 \text{ C}'. \text{ (Mean.)} \end{aligned}$$

Hence, since Zn | Cu = 0·7892 D = 0·8516 volt,

$$\text{C}' = 1{\cdot}343 \text{ D} = 1{\cdot}449 \text{ volts.}$$

On March 13 and 15, 1877, Clark's cell was directly compared with a Daniell's cell, giving

$$C' = 1{\cdot}192\ \text{D.}$$
$$1{\cdot}178\ \text{D.}$$
$$\overline{1{\cdot}185\ \text{D. (Mean.)}}$$

On March 28, 1877, the same comparison was repeated, leading to the result

$$C''' = 1{\cdot}169\ \text{D.}$$

Hence it appears that the difference of potential exhibited by the terminals of Clark's standard cell is constantly diminishing during the period of these experiments, being

	Standard Daniell.	Volt.
On December 15–16, 1876..................	1·343	1·449
„ January 5–8, 1877	1·273	1·374
„ March 13–15, 1877	1·185	1·279
„ March 28, 1877	1·169	1·262

In the course of my experiments I have had occasion to compare the difference of potential exhibited by the copper terminals of various voltaic elements with the difference of potential of the terminals of a standard Daniell's cell, in which the amalgamated zinc plate is immersed in a liquid composed of 1 part by weight of pure sulphuric acid and 4 parts by weight of distilled water. Some of the results have been already mentioned, and the whole are contained in the Table accompanying this paper, arranged with the differences of potential in ascending order of magnitude.

In all the cells referred to in this Table the terminals have been carefully insulated, so that in no case has a current circulated in any of the cells; the difference of potential observed is therefore free, or nearly free, from the influence of the phenomenon known as the polarization of the metal plates.

I hope shortly, with somewhat improved apparatus, to undertake a more extended series of measurements of the differences of potential due to the contact of metals, and metals and liquids, and by using purer metals to be able to present to the Society results of more value than those contained in the present paper.

Table showing the Difference of Potential of the Copper Terminals of Voltaic Elements in which no Current has circulated.

Composition of Element.	Difference of Potential in terms of	
	Standard Daniell.	Volt.
Tin in dilute sulphuric acid (1 volume of acid to 8 volumes of distilled water) and Tin in solution of caustic potash (1 part by weight of caustic potash to 5 parts by weight of distilled water)	0·532	0·574
Tin in distilled water and Tin in solution of caustic potash (1 part by weight of caustic potash to 5 parts by weight of distilled water)	0·713	0·769
Copper and zinc (both well cleaned) in distilled water:—		
When metals are first immersed	0·760	0·820
After immersion for 1·5 hour	0·821	0·886
" " " 3 hours	0·838	0·905
Copper and amalgamated zinc in dilute sulphuric acid (1 volume of commercial sulphuric acid to 8 volumes of distilled water)	0·856	0·924
Copper in distilled water and Copper in solution of potassium cyanide (1 part by weight of potassium cyanide to 5 parts by weight of distilled water)	0·923	0·996
Daniell:— Copper in saturated solution of copper sulphate and amalgamated zinc in dilute sulphuric acid (1 volume of commercial sulphuric acid to 10 volumes of distilled water)	0·958	1·034
Daniell:— Copper in saturated solution of copper sulphate and amalgamated zinc in dilute sulphuric acid (1 volume of commercial sulphuric acid to 8 volumes of distilled water)	0·982	1·059
Carbon and zinc in a saturated aqueous solution of potassium bichromate	0·996	1·074

TABLE (*continued*).

Composition of Elements.	Difference of Potential in terms of	
	Standard Daniell.	Volt.
Copper in saturated solution of copper sulphate and copper in solution of potassium cyanide (1 part by weight of potassium cyanide and 5 parts by weight of distilled water)	1·102	1·189
Smee:— Platinized silver and amalgamated zinc in dilute sulphuric acid (1 volume of commercial sulphuric acid to 8 volumes of distilled water) ..	1·193	1·288
Leclanche:— Solution of ammonium chloride in distilled water	1·268	1·369
Grove:— Platinum in commercial nitric acid (sp. gr. 1·36) and amalgamated zinc in dilute sulphuric acid (1 volume of commercial sulphuric acid to 12 volumes of distilled water)	1·504	1·622
Platinum in acid solution of potassium bichromate (4 volumes of saturated aqueous solution of potassium bichromate to 1 volume of commercial sulphuric acid) and Amalgamated zinc in dilute sulphuric acid (1 volume of commercial sulphuric acid to 8 volumes of distilled water)	1·678	1·811
Carbon and zinc in acid solution of potassium bichromate (4 volumes of saturated aqueous solution of potassium bichromate to 1 volume of commercial sulphuric acid)	1·701	1·835

V. "The Physiology of Sugar in relation to the Blood." By F. W. PAVY, M.D., F.R.S. Received June 12, 1877.

In a communication published in the 'Transactions of the Royal Society' (1860, p. 579) I gave the results of analyses showing that what had previously been looked upon, under Bernard's glycogenic theory, as the natural condition of the blood in relation to sugar was a fallacious representation due to a *post mortem* change being allowed to exert its influence. It had hitherto been asserted that the blood of the right side of the heart was in a notably different condition as regards the amount of sugar it contained from that of the arterial system, an error which I discovered arose from the non-observance of certain precautions in the mode of obtaining the blood for examination from the respective parts of the vascular system. Whilst the arterial blood had been collected during life, it was customary to collect that from the right side of the heart, without any special haste, after the destruction of the life of the animal. During the period thus allowed to elapse between the moment of death and the collection of the blood, an alteration occurs from the *post mortem* production of sugar in the liver, which causes the blood to assume an extent of saccharine impregnation which does not naturally belong to it during life, and which had failed to be recognized in its true light. I gave analyses which show that what was formerly taken as representing the natural condition of the blood of the right side of the heart furnished from ·50 to ·94 per cent., or, as it is more convenient to state it, 5·0 to 9·4 per 1000 of sugar, the blood from the carotid artery of the same animals, collected during life, having contained what I described as a trace of sugar. Other analyses, three in number, were given, representing the true condition of the blood belonging to the right side of the heart during life, and the results indicated from ·47 to ·73 per 1000 as the amount of sugar.

Bernard has recently published some communications entitled "Critiques expérimentales sur la glycémie," in the Comptes Rendus de l'Académie des Sciences de Paris. His statements are founded upon a method of analysis which is not only strikingly devoid of precision as a quantitative analytical process, but in itself of a nature calculated to give rise to a fallacious result.

The process adopted by Bernard is as follows:—He makes use of a Fehling's solution, titrated to render 1 c. c. equivalent to 5 milligrammes of sugar. He withdraws with a syringe, or collects as it flows from the vessel in a weighed porcelain capsule, a determined quantity—10, 15, or 25 grammes of blood. To this he adds an equal weight of sulphate of soda in small crystals, with a few drops of acetic acid, and heats immediately over the flame of a gas-burner or spirit-lamp, to coagulate the albuminous and colouring matters. On account of the relatively small quantity of sugar to be dealt with, he only uses for the analysis 1 c. c. of the standard copper solution. This he heats in a small glass flask, after having added 20 to 25 c. c. of a fresh concentrated solution of potash, and drops into it the liquid to be tested till decolorization is effected. From the suboxide remaining dissolved, and the liquid being thus free from precipitate, the attainment of the point of decolorization is easy to be perceived. Observation, he says, has shown that the relation of volume of liquid yielded to weight of a mixture of equal parts of blood and sulphate of soda is $\frac{4}{5}$; in other words, that 50 grms. of sulphate of soda and of blood give 80 c. c. of trial liquid. The estimation having indicated how much sugar each c. c. of this liquid contains, the data are afforded for ascertaining the quantity of sugar in the volume corresponding with the weight of blood analyzed, and thence the ratio per 1000.

There are two seriously faulty points about the method as a quantitative process of analysis.

The first is the assumption that the volume of the trial liquid corresponds in c. c. with $\frac{4}{5}$ of the weight in grms. of the mixture of sulphate of soda and blood. In reality, the actual relation between the volume of liquid obtained and the weight of the mixture employed must vary in each individual instance with the proportion of solid matter existing in the particular specimen of blood, and the loss of liquid by evaporation from the capsule during the process of coagulation by heat. It may be regarded as totally impossible to secure that the coagulation of the albuminous and colouring matters can be effected, in an open capsule at the high temperature necessary, with identically the same loss by evaporation in each individual instance; and as the amount of liquid derivable from the mixture is not large, a slight variation must alter to a decided extent the result, especially when it is worked out into the proportion in 1000 parts. With 20 grms. of blood as the quantity submitted to analysis, any error existing in the result yielded becomes multiplied 50 times when the representation of ratio per 1000 is made.

The other point is, that the process involves the influence of organic matter in preventing the deposition of the suboxide of copper. It is not the usual principle of applying the test that is appealed to. In the ordinary volumetric process of analysis with the copper solution, attention is given to the separation of suboxide till the whole of the copper has been removed from the liquid. In Bernard's process a large addition of potash is employed (20 to 25 c. c. of a concentrated solution to 1 c. c. of the copper test), the effect of which is to act upon some one or other organic principle left in the liquid obtained from the blood, and prevent a deposit of suboxide occurring. The presence of the potash does not by its own action interfere with the fall of suboxide; for when the process is carried out upon a simple solution of sugar, the suboxide falls in the usual way, whilst in the case of the liquid obtained from the blood, the removal of blue colour takes place without any signs of appearance of precipitate. Without the addition of the potash the usual behaviour occurs.

Actual observation shows that the results yielded by Bernard's process are very wide of those given by a process to be presently described, which is founded upon the precipitation of the suboxide and the subsequent collection of the copper by means of galvanic action upon a cylinder of platinum foil, as is now extensively done in the assaying of copper ores. This application of the copper solution yields a gravimetric *instead of a volumetric* process of analysis. There is nothing in it of a doubtful nature. The result being given by the balance, there is no uncertainty belonging to it, as may be the case to a slight extent where the gradual fading of colour has to be watched and a decision formed with regard to the attainment of the precise point. If the reduction of the oxide of copper can be safely turned to account for effecting the quantitative determination of sugar (and analysts are agreed that it can), it is by such process that the most trustworthy information is supplied. The closeness attainable in the results of counterpart analyses shows that it there is not only susceptible of being carried out with great precision, but affords strong evidence of its reliability. Compared with this process, the results yielded by that of Bernard present the greatest discordancy. The figures given by Bernard's method are almost invariably too high, but there is no uniformity in the difference presented. The variance, indeed, shows no intelligible relation, and suggests that there is something fundamentally wrong in taking decolorization, without precipitation of the suboxide, as a means of estimating the amount of sugar. Subjoined are the results of the analyses of different specimens of blood in which Bernard's process has been compared with my own. In every case where my own process of analysis is put into practice I submit two samples of the blood to examination, in order that the results may be checked, and the same plan was adopted in some of the trials of Bernard's method. In these instances, therefore, we have the results derived from the analysis of four separate samples of the same blood, taken for examination at the same time:—

Source of Blood.	Sugar per 1000 parts.			
	Gravimetric process.		Bernard's volumetric process.	
I. From bullock killed by Jewish method	*a.* ·589 *b.* ·588	·588 (mean).	·975	
II. From bullock killed by Jewish method	*a.* ·510 *b.* ·489	·499 (mean).	*a.* ·609 *b.* ·640	·624 (mean).
III. From bullock killed by Jewish method	*a.* ·515 *b.* ·535	·525 (mean).	1·025	
IV. From bullock killed by Jewish method	*a.* ·698 *b.* ·709	·703 (mean).	·869	
V. From bullock killed by pole-axe	*a.* 1·091 *b.* 1·097	1·094 (mean).	*a.* 1·311 *b.* 1·403	1·357 (mean).
VI. From jugular vein of dog, instantly after death	*a.* ·795 *b.* ·811	·803 (mean).	·800	
VII. From sheep	*a.* ·509 *b.* ·526	·517 (mean).	·761	
VIII. From case of severe diabetes, obtained by cupping	*a.* 4·990 *b.* 4·951	4·970 (mean).	*a.* 5·000 *b.* 4·705	4·852 (mean).

The gravimetric process of analysis to which I have referred consists of three stages. The blood is first mixed with the sulphate of soda and heated to separate the albuminous and colouring matters; the liquid is then separated and the coagulum well washed to remove all the sugar. Boiling with an excess of the copper solution is next performed, and the reduced oxide afterwards collected and dissolved by the agency of an acid. In this solution a cylinder of platinum foil is immersed, for the purpose of receiving the copper removed by means of galvanic action. Weighing the platinum foil before and after the operation gives the weight of the deposited copper; and from this may be calculated the amount of sugar which has effected the reduction of the cupric oxide. Such is an outline of the process; but a special description of the manipulation is required, as success is dependent upon certain details being closely followed. In the application of the process many difficulties at different stages presented themselves; but it is satisfactory to be able to state that they have all been overcome, and there is reason to believe that no source of fallacy now exists in any part of the operation.

Forty grms. of sulphate of soda in small crystals are weighed out in a beaker of about 200 c. c. capacity. About 20 c. c. of the blood intended for analysis are then poured upon the crystals, and the beaker and its contents again carefully weighed. In this way the precise weight of the blood taken is ascertained. The blood and crystals are well stirred together with a glass rod, and about 30 c. c. of a hot concentrated solution of sulphate of soda added. The beaker is placed over a flame guarded by wire gauze, and the contents heated till a thoroughly formed coagulum is seen to be suspended in a clear colourless liquid, to attain which actual boiling for a short time is required. The liquid has now to be separated from the coagulum, and the latter washed, to remove all the sugar. This is done by first pouring off the liquid through a piece of muslin resting in a funnel into another beaker of rather larger capacity. Some of the hot concentrated solution of sulphate of soda is then poured on the coagulum, well stirred up with it, and the whole thrown on the piece of muslin. By squeezing the liquid is expressed; and to secure that no sugar is left behind the coagulum is returned to the beaker, and the process of washing and squeezing repeated.

The liquid thus obtained may be fairly regarded as containing all the sugar that existed in the blood. From the coarse kind of filtration and the squeezing employed, it is slightly turbid, and requires to be thoroughly boiled to prepare it for filtration through ordinary filter-paper. A perfectly clear liquid freely runs through; and to complete this part of the operation, the beaker in use and the filter-paper are washed with the concentrated solution of sulphate of soda before referred to.

The next step is boiling with the potassio-tartrate of copper test solution. The liquid is again placed over a flame, and brought to a state of ebullition. A sufficient quantity of the copper solution to leave some in excess is now poured in, and, from the commencement of boiling again, *brisk ebullition is* allowed to continue for the full space of one minute.

This suffices for all the sugar to be oxidized, and accordingly for all the required action upon the copper solution to occur. There is no risk during this time of spontaneous change occurring in the copper solution; but observation has shown that should boiling be continued for a lengthened period (by which I mean ten minutes or a quarter of an hour) the copper solution undergoes alteration, and no longer possesses the power of resisting spontaneous reduction. As regards the amount of copper solution, 10 c. c. of the test of ordinary strength are found to be more than sufficient for 20 c. c. of the blood of animals in a natural state. Where from any cause an extra quantity of sugar exists, more in proportion of the test-solution is of course required.

The precipitated suboxide of copper has now to be separated from the excess of copper solution. Experience shows that filtration through filter-paper cannot be resorted to for the purpose. In the first place, the pores of the paper tend to become blocked up and filtration to be stopped; and in the next (and this is a fatal objection) the paper absorbs and so tenaciously holds some of the copper solution that it cannot be effectually washed out. A plug of asbestos, in a filter-funnel, may be used instead; but it is not always easy to procure the asbestos with fibres of the medium state of fineness to answer well for speedy and, at the same time, delicate filtration. A material, however, which has recently been introduced, viz. glass-wool, exactly furnishes what is wanted. Properly packed in the neck of a funnel, it permits filtration to be effectively and speedily performed; the state of the filtrate readily shows if the plug has not been sufficiently closely packed to keep the whole of the precipitate back.

Should the crystallization of the sulphate of soda in this or the preceding filtration interfere with the process of filtration, the funnel may be lodged upon a beaker containing fluid kept in a state of ebullition. Through the heat thus applied, the liquid is prevented from assuming a crystalline form.

The suboxide having been collected and washing with distilled water performed, it is returned to the beaker in which the reduction was effected, to secure that whatever precipitate may have been adhering to the sides of the vessel is retained. The plug is simply pushed with a glass rod from the funnel inverted over the beaker, and the funnel washed and its surface cleaned from all adhering precipitate. We have now the suboxide in a fit state to dissolve; and until I resorted to the use of peroxide of hydrogen to effect its oxidation a difficulty presented itself in this part of the operation, the precipitate requiring an amount of acid to dissolve it which interfered with the subsequent deposition of copper by galvanic action. After the addition of a few drops of peroxide of hydrogen, a very small quantity of nitric acid (a few drops only) is sufficient to lead to instantaneous solution; and after boiling to decompose the excess of peroxide of hydrogen, the contents of the beaker, consisting of the filter-plug and dissolved precipitate, are poured into a funnel containing a loose plug, to obtain the liquid in a separate form. The requisite washing with distilled water having been performed, there only remains the final stage of the process to be conducted.

The liquid to be now dealt with contains the copper in the form of nitrate, which experiment has shown to be the most suitable for yielding a pure metallic deposit by galvanic action. For the purpose of collecting the deposit, a cylinder of platinum foil soldered to a platinum wire, for hooking on to the negative pole of the battery, is employed. This is immersed in the liquid so as nearly to touch the bottom of the vessel, and inserted within it is a spiral coil of platinum wire, made to form the positive pole of the battery. In order to secure a good continuous connexion, the platinum spiral is closely bound to the copper conducting-wire of the battery, and the other pole is provided with a platinum hook, for the suspension of the cylinder. This precaution has been found necessary from the ready manner in which copper exposed ends become oxidized and rendered imperfect conductors by the oxygen escaping from the liquid underneath. It may be also mentioned that the platinum spiral, after several days' use, presents a brown surface, and requires to be occasionally cleaned by immersion in hydrochloric acid. In the case of an analysis of blood containing an ordinary amount of sugar, and therefore yielding a limited amount of copper to be deposited, twenty-four hours have been usually found to thoroughly suffice for complete removal to occur: but it is necessary that there should be no uncertainty upon this point; and to provide against this the liquid must be tested before the operation is regarded as finished. A small quantity is taken from the *bottom* of the vessel by means of a pipette, and a little ammonia added to it in a test-tube. Should a decided blue colour be produced it should be returned to the vessel; should no blue colour be perceptible the testing must be carried further. Acetic acid is added in excess to the contents of the test-tube to supersaturate the ammonia, and then a small quantity of a solution of yellow prussiate of potash dropped in. In the absence of copper, nothing but the faint yellow colour of the test is perceptible; but with the presence of copper a brownish hue is produced, and the galvanic action must be carried on till, on testing, this is no longer brought out. When it is thus found that the whole of the copper has been thrown down, the cylinder is lifted quickly out of the liquid and instantly plunged, first into distilled water and then into spirit. After drying in a water-oven, it is ready for weighing, and it need hardly be said that a delicate balance is required for the purpose. The weight of the cylinder before and after the operation indicates the amount of copper deposited.

The galvanic action requires to be steadily and continuously maintained, and a modification of Fuller's mercury-bichromate battery has been found to be highly suitable for use. The arrangement that has been employed in my experiments consists of an outer cell charged with bichromate of potash dissolved to saturation in dilute sulphuric acid. In this two carbon plates are immersed. The inner porous cell contains a little mercury at the bottom, but is otherwise filled up with water. An amalgamated zinc rod is inserted, and dips down into the layer of mercury. This battery, it is found, gives a steady current, and, used every day, will remain in good working order for at least a fortnight, all that is necessary being to pour out the liquid in the porous cell when it has become green from reduction of the diffused bichromate solution and replace it with water. Attention is of course necessary to secure that the proper battery-power exists to effect the deposition of the copper; and when the current becomes weak the zinc rod must be cleaned and the bichromate of potash solution replenished.

The relation existing between sugar oxidized and cupric oxide of the copper test solution reduced is, that one atom of the former reduces five atoms of the latter. This is the foundation upon which the action of the test is based, and the calculation made in estimating by its agency the amount of sugar present. Taking 63·4 as the atomic weight of copper, and 180 as that of glucose ($C_6H_{12}O_6$), 317 parts of copper will stand equivalent to 180 parts of glucose. Thus one part of copper corresponds to ·5678 of glucose; and in calculating the amount of sugar in the blood analyzed, the weight of the copper deposited has only to be multiplied by ·5678 to give its equivalent in glucose. The quantity of sugar in the amount of blood taken for analysis being thus determined, the data are furnished by which the proportion per 1000 parts may be ascertained.

I have entered thus minutely into the description of the gravimetric process of estimating sugar, as I feel that it supplies a new mode of investigation which is calculated to materially advance our position with reference to the physiological relations of sugar in the animal system. It has hitherto always seemed to me that in giving numerical representations of minute quantities of sugar they could only be regarded as approximate results. Now, however, sufficient precision is attainable to enable minute differences to be ascertained with the requisite certainty for definite physiological conclusions to be drawn.

In a further communication, to be presented for the next meeting of the Society, I propose to give the results I have already obtained bearing on (1) the natural state of the blood; (2) the comparative states of arterial and venous blood; and (3) the spontaneous change ensuing after the removal of blood from the system.

VIII. “On the Length of the Spark between two Spherical Surfaces of the Chloride-of-Silver Battery.”

By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. Received June 14, 1877.

In anticipation of a detailed account of our researches in voltaic electricity, which have engaged our attention for nearly three years, we venture to publish, as of interest to the electrician, an account of the results obtained with two spherical surfaces of 3 inches radius and 1·5 inch diameter. It will be seen that they differ materially from those which occur from the employment of a point for one terminal and a flat disk for the other, with which the striking-distance is in the ratio of the square of the number of cells up to 8040, as has already been stated to be the case up to 2400 cells (Proc. Roy. Soc. no. 166, 1876).

The Ag Cl cell is taken as equal to 1·03 volt in the calculations from which the following numbers are derived :—

Difference of potential in volts.	Length of spark in air at the atmospheric pressure.	Additional length of spark for 1000 additional volts.	
	in.		in.
250	0·00100	between 0 and 1000	0·00482
500	0·00225		
750	0·00350		
1000	0·00482	„ 1000 „ 2000	0·00751
1500	0·00820		
2000	0·01233	„ 2000 „ 3000	0·00967
2500	0·01700		
3000	0·02200	„ 3000 „ 4000	0·01025
3500	0·02700		
4000	0·03225	„ 4000 „ 5000	0·01100
4500	0·03775		
5000	0·04325	„ 5000 „ 6000	0·01135
5500	0·04900		
6000	0·05460	„ 6000 „ 7000	0·01190
6500	0·06070		
7000	0·06650	„ 7000 „ 8000	0·01200
7500	0·07250		
8000	0·07850		

It is evident that, for small distances between the terminals, a higher difference of potential is necessary to cause the spark to jump than when they are at greater distance. This agrees with Sir William Thomson's experience.

We avail ourselves of this opportunity to state that, when observed with the microscope, the voltaic arc, at ordinary atmospheric pressures, is seen to be stratified, though with some difficulty.

Also that the origin of all strata in exhausted tubes is at the positive pole. At certain pressures there is only one stratum, then, as the pressure is diminished, two, three, and so on, each being added on from the positive pole. We succeed easily in obtaining photographs of the phenomena, as the strata can generally be made to remain stationary for some time. Several of the photographs are in the hands of the engraver to be copied, and we hope to be able to show the history of several tubes in a communication we are now drawing up.

June 21, 1877.

Sir JOSEPH HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. John Duke Lord Coleridge and Dr. Thomas Richard Fraser were admitted into the Society.

The President announced that Section V. Chapter III. of the Statutes, under which a Fellow whose paper had been printed in the Philosophical Transactions could claim to pay, in lieu of the annual contribution, a Life Composition of £40 instead of £60, had been repealed by the Council.

The following Papers were read:—

I. "On the Normal Paraffins." Part II.—By C. S. Schorlemmer, F.R.S., Professor of Organic Chemistry in Owens College, Manchester. Received June 5, 1877.

(Abstract.)

In the first paper of this subject it was shown that by the action of chlorine on a normal paraffin a primary chloride and a secondary one of the general formula $\left.\begin{matrix}C_nH_{2n+1}\\CH_3\end{matrix}\right\}CHCl$ are formed simultaneously[*]. It appeared of interest also to examine the action of bromine on the paraffins. The present paper contains the first results of this research.

I. *Normal Hexane.*—When bromine vapour is passed into the vapour of the boiling hydrocarbon, in the daylight, its colour disappears quickly and substitution products are formed which are partly decomposed by distillation. The portion distilling without decomposition consists of a hexyl bromide, which was converted into the acetate and the alcohol. The latter yielded on oxidation *acetic acid* and *normal butyric acid*, showing that it is *methylbutyl carbinol*. The boiling-points of the different compounds are as follows:—

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[*] Phil. Trans. vol. 162, part i. p. 111.

	Formula.	Boiling-point.
Bromide	$\left.\begin{matrix}C_4H_9\\CH_3\end{matrix}\right\}CHBr$	143°–145°
Acetate	$\left.\begin{matrix}C_4H_9\\CH_3\end{matrix}\right\}CH.OC_2H_3O$	146°–150°
Alcohol	$\left.\begin{matrix}C_4H_9\\CH_3\end{matrix}\right\}CH.OH$	136°–140°

II. *Normal Heptane* yielded the following products:—

Bromide	$\left.\begin{matrix}C_5H_{11}\\CH_3\end{matrix}\right\}CHBr$	165°–167°
Acetate	$\left.\begin{matrix}C_5H_{11}\\CH_3\end{matrix}\right\}CH.OC_2H_3O$	169°–171°
Alcohol	$\left.\begin{matrix}C_5H_{11}\\CH_3\end{matrix}\right\}CH.OH$	155°–157°

The heptyl alcohol is *methylpentyl carbinol*, because on oxidation it was resolved into *acetic acid* and *normal pentylic acid*.

As result of this investigation it appears that by the action of bromine on normal paraffins only secondary bromides of the general formula $\left.\begin{matrix}C_nH_{2n+1}\\CH_3\end{matrix}\right\}CHBr$ are formed, but not a trace of a primary bromide, or that the methyl groups which are present in these hydrocarbons, and which are readily attacked by chlorine, are not touched by bromine at all.

In addition to the secondary bromides other products are formed which, on distillation, decompose either completely or are resolved into hydrobromic acid and non-saturated hydrocarbons, which are probably olefines. By continuing this research I hope to ascertain the nature of these non-volatile products.

II. "The Relationships of the Nerve-cells of the Cortex to the Lymphatic System of the Brain." By Bevan Lewis, F.R.M.S., Assistant Medical Officer at the West Riding Asylum. Communicated by Dr. Ferrier, F.R.S. Received June 8, 1877.

[Plates 1 & 2.]

The great importance attached to an accurate appreciation of the relationships existing between the nerve-cells and the lymphatic and vascular systems in the brain cortex will be recognized by all who are engaged upon investigations in cerebral pathology, and cannot be well overestimated. These anatomical relationships have had great attention bestowed upon them by continental histologists, and more especially those of the German school. Amongst the more important subjects in which their acumen has served to enlighten us we may take, as an illustration, the demonstration of the intimate connexion existing between the lymph-sacs and perivascular channels of the brain, and the successful injection of the former by Obersteiner*. Although several years have elapsed since the publication of Obersteiner's views, the accuracy of his statements has not received that appreciation and acknowledgment by English observers which the importance of the subject imperatively demands, nor does it appear that a critical examination of these "pericellular spaces" has been instituted with the object of finally setting the question at rest.

In his work on this subject Obersteiner's views are expressed so clearly, and the illustrations are so definite, that little doubt as to their accuracy can, I think, remain on the mind of the unprejudiced reader.

That his views have not been generally accepted amongst us, I infer from the frequent confusion traceable in the writings of many pathologists in this country with regard to the relationships of the nerve-cells of the cerebral cortex to the immediate environment, from the dubiousness expressed by others as to the nature of these spaces when observed by them, and, finally, to the error committed by others of referring them to an entirely different source to that which Obersteiner claims for them.

These are the reasons which have induced me to describe my own investigations on the subject and state my opinion, which, on most points, is strictly in conformity with that of the German observer—full opportunities having been afforded me for confirming his statements and gauging their value in pathological researches.

Existence of Pericellular Lymph-sacs in the Brain.

My attention was first attracted to the significance of these spaces by (*a*) the prevalence, in certain morbid conditions, of numerous nuclei arranged in definite directions around the nerve-cells, (*b*) the presence of undoubted lymph-corpuscles in clear spaces around the cell, and (*c*) the appearance of pericellular spaces in healthy brain occasionally where the cells appeared perfectly normal and certainly not atrophic.

The arrangement of nuclei, above alluded to, is at times most striking, and is especially well seen in the larger nerve-cells of the third layer and the still larger cells found at a lower level in the ascending frontal and ascending parietal convolutions of man, which have been termed giant cells. These cells are undoubtedly *normal* constituents of the cortical layers, and to a great extent *constant* elements in these regions. I have represented one of these great nerve-cells in Plate 2. fig. 4. To appreciate the significance of this arrangement of nuclei around these cells it will be necessary shortly to consider the actual cell-elements which enter into the formation of the non-nervous portion of the neuroglia. These elements are usually stated to consist of free nuclei and distinct nucleated cells. The nuclei of the latter are said by Meynert,* to have a diameter of 9–10μ (=·009 to ·010 millim.), a measurement which coincides with my own. I find these last-mentioned cells possess an extremely delicate protoplasm, so readily affected by reagents that it is easily altered in appearance or entirely masked from view. Within these cells two nuclei are frequently seen. These cells, in certain morbid conditions, proliferate freely, and throw out delicate and anastomosing branches in all directions, assuming the form of "Deiter's cells." They are undoubtedly connective elements, and by this morbid proliferation they produce a true sclerosis of the cortex; such a condition is delineated in Plate 2. fig. 5. This morbid growth of the nucleated cell in the neuroglia is alluded to by Meynert as bearing a relationship to arrest in the discharge of the perivascular lymph, as would occur in hyperæmia and also in degeneration of the lymphatic glands of the head and neck. I have myself frequently observed these changes in tuberculosis and senile atrophy of the brain. It must be borne in mind that such a growth must still further impede the current in the perivascular channels by the intertwining of their numerous branches around the walls of the delicate capillaries of the cortex.

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* "Ueber einige Lymphräume im Gehirne" (Sitzb. d. k. Akad. d. Wissensch. 1 Abth., Jän.-Heft, 1870).

It is important that a distinction be drawn clearly between these cells and the other nuclear elements referred to. These, the so-called free nuclei, are not in reality *free* nuclei; for when examined carefully in the fresh state they are found to have a slight delicate investment of protoplasm, which becomes quite destroyed by subsequent methods of preparation. These nucleus-like bodies resemble, as regards size, form, structure, and reaction to staining and chemical reagents, those which are found universally scattered through the brain, and which most observers, I believe, accept as representatives of the connective-tissue series. Now these elements appear to me to be disposed in three definite situations:—*a*, irregularly in the neuroglia framework; *b*, regularly around the nerve-cells; *c*, following directly the course of capillaries.

In the two last positions they are connected with the lymph-walls surrounding the blood-vessels and nerve-cells, and I am disposed to regard them as endothelial elements lining these channels. The spindle-cell of the deepest cortical layer in the frontal region appears to be especially prone to the growth of these attendant satellites, which accumulate occasionally in such numbers as to form superincumbent heaps, almost concealing the nerve-cell from view.

Frequently the capillary is so faintly stained that we can trace its course alone by the line of nuclei running parallel to its wall, and within which the elongated nucleus proper to the vascular coat becomes now and again apparent.

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* Stricker's 'Histology' (Sydenham Society), vol. ii.

Identical as these nuclear bodies are in appearance their site probably betokens far different functional endowments appertaining to each, the extravascular being truly connective, whilst those lining the lymphatic channels develop into lymph-corpuscles, and correspond, in fact, to lymphatic endothelia. These connective and endothelial elements having a separate and wholly distinct destiny, also differ *inter se* in relative number and distribution, the connective elements being, in conditions of health, a constant element, the endothelial subject to diverse physiological influences, by which their appearance and proportions may be greatly modified. The recognition of these connective and endothelial elements, and the varying conditions imposed upon them by their distinct functional endowments, is of essential importance when we are dealing with the morbid brain.

Further examination sufficed to prove that this arrangement of nuclei regularly around the nerve-cell in the human subject, in which epilepsy and other morbid affections were present before death, was a condition frequently met with in the lower animals as apparently a perfectly normal state, and the brain of the healthy cat afforded peculiarly favourable opportunities for studying their distribution and significance.

Here it was most satisfactorily demonstrated to my mind that the pericellular nuclei were arranged along the boundary of a perfectly clear space. This space completely enclosed the nerve-cell and gradually tapered off and disappeared towards the apex process of the pyramids. The remaining processes (lateral and basal) of the nerve-cell cross the intermediate space, passing through the boundary wall of the enclosing sac, the cell being in reality suspended by its branches within this pericellular cavity. The appearance of these spaces will be greatly modified by the method of preparation adopted, the thinness of the section, and various physiological and pathological conditions existing before death.

On reference to the sketch (Plate 1. fig. 1) which represents the ascending frontal convolution in a young cat, the section being taken from the gyrus immediately in front of the crucial sulcus, we find four nerve-cells surrounded by these clear spaces which, in this case, are widely distended. One of these pericellular sacs also exhibits the arrangement of nuclear elements above alluded to. In most cases the outline of the cell is closely followed by the enclosing sac, and from this cause the form of the pericellular sac is subject to great diversity of contour.

Recent examination of the cerebral cortex in sections cut on the freezing microtome has assured me that the nerve-cells of the middle layers of the cortex are subject to no definite rule as regards their form. The pyramidal form was too exclusively supposed by Meynert to be due to the action of hardening agents, and he certainly erred in assuming the spindle form to be the natural and most prevalent. The fact seems to be that the forms are too protean for any such exclusive statement to be scientifically accurate. The fusiform cell is frequently seen in the third layer, the pyramidal is also frequent in fresh specimens, others are pyriform, ovate, globose, or even so indefinite as to warrant the term amœbiform.

It will thus be apparent that the lymph-space surrounding the cell must differ greatly in its contour. In some cases the lymph-sac is not apparent, although its endothelial elements may indicate its existence; this, in all cases, I believe, is due to the mode of preparation, or to pathological changes (probably, also, certain physiological conditions) not yet satisfactorily investigated. Within these spaces we can frequently detect the corpuscular elements of the lymphatic system, whilst a faintly granular plasma remains as the representative of the coagulated lymph. The boundary wall of these spaces varies much in appearance, according to the character assumed by the surrounding matrix of neuroglia. The more finely granular the appearance of the neuroglia the less perfectly can the wall of the pericellular space be differentiated from its environment, the minutest fibrils of connective and nerve intermingled in the finely granular basis approaching to the clearly defined margin of the space, and no distinct limiting membrane being observable. In all probability these spaces are lined merely with a delicate endothelial investment. In cases where great shrinking of structure has occurred by the use of strong solutions of chromic acid and subsequent methods of preparation, I have observed much distortion occur, whereby the cell is laterally displaced, or even partially withdrawn from its enclosing sac. The next stage in my observations was arrived at by the discovery that a minute blood-vessel invariably ran in close contact with all the large nerve-cells. In some cases the elongated nuclei of the capillary might not have been sufficiently stained and the outline of the vessel not distinct, yet the line of perivascular endothelial elements would unmistakably indicate its course, except where the vessel had been cut across, when the open lumen surrounded by its perivascular sheath, often with one or more nuclei attached, still indicated the close proximity of the vessel to a nerve-cell.

In all cases I have never failed to recognize, on careful examination, a small capillary either passing immediately across the nerve-cell or running with a gentle curve along the confines of the pericellular space. The invariable occurrence of this arrangement naturally struck my attention as a highly significant fact, and more extensive observations proved it to be the universal arrangement throughout the cortex cerebri. In many instances a distinct connexion between perivascular and pericellular space could be clearly observed, although from obvious reasons the majority of specimens exhibited this connexion only after the most careful and strict scrutiny, or afforded, on the other hand, no definite *indications of its* existence.

The fact, however, of a direct communication existing between these spaces is established when seen clearly and unmistakably in a single instance. In the accompanying sketch (Plate 1. fig. 1) such a connexion is well shown, and I can fully vouch for the accuracy of the minutest details here portrayed.

With regard to the mode of connexion it must be remembered that the pericellular sacs are laterally disposed along the sides of the smaller capillaries, and in no case occupy a terminal position; hence the nerve-cell is bathed in a constantly renewed current of lymph on all its sides. Does this distribution in any way indicate the mode of development of the nerve-cell? With the object of answering this query I examined several brains of fœtal and adult animals; and although I cannot speak conclusively on the point, it will be apparent, by reference to figs. 2, 3, that the characters represented in such sections strongly confirmed the views adopted above, and indicate likewise a development of nerve-cells from within the perivascular sheath projecting from its walls in egg-shaped ampullæ. The nerve-cells are seen in these specimens to follow definitely the course of the blood-vessels, and often surround the latter in crowds, and assume with the direction of the vessel a linear or arched course. In the cortex of the new-born kitten the appearances indicated in fig. 2 were seen. The perivascular sheaths were unusually defined, and could be traced along the most minute capillaries. The nerve-cells were arranged in one or two series along the course of the capillary, and are clearly separated by a space from the neuroglia in its vicinity. The cells and their limiting sacs are somewhat pyriform, and appear to arise by narrowed stalk-like processes. The pyriform appearance is still better marked in a section from the ascending frontal convolution of a young dog (fig. 3), which shows two large pyriform cells, distinctly nucleated and within their enclosing sheath. At this period it will be observed that the nerve-cells have not thrown out their processes, and the development can be traced merely from a globular cell or nucleus to a distinctly nucleated pyriform cell. Such appearances would seem to indicate that the nerve-cells of the cerebral cortex are lymphatic outgrowths; but whether this be so, or whether amœboid vagrants from the blood-current be the source of these centres of nervous activity, must remain for a time *sub judice.* One important fact it is well to bear in mind; the existence of these pericellular sacs enables us to infer a more direct organic connexion between vessel and nerve-cell than we were capable of assuming before. Further research alone will enable us to establish the existence of that mutual sympathy between nerve-cell and vascular contents in healthy and diseased action which is dependent upon their development from similar and identical elements.

Important considerations must be drawn if this view of the anatomical relationship of nerve-cells to a lymphatic system be generally accepted. There are observers who have attributed these spaces, when seen in senile atrophy, to mere shrinking of the cell from degenerative changes, and the production thereby of an artificial fissure or chasm around the cell. The broader views now advanced will enable such observers to expect similar appearances from very diverse causes.

The unusual distinctness with which these lymph-sacs sometimes present themselves may, I am convinced, be occasioned by hardening agents, such as chromic acid and its salts; but I feel equally well assured that similar appearances may result from an unusually distended lymph-system. We may therefore expect to find them well shown where obstructive agencies interfere with the outflow of the lymph-stream towards the pia mater. The large size and defined contour of these lymph-sacs in senile atrophy of the brain may undoubtedly be due to shrinking of the enclosed degenerating protoplasm of the cell; yet the important point is to recognize these spaces as *natural* structures in an unnaturally distended condition, for their large size appears to me due not only to wasting and recession of the enclosed cell, but to a large accumulation of lymph, the lymphatic channels, both pericellular and perivascular, being in a distended condition throughout.

Sections from the frontal lobe of a *young* and perfectly *healthy* cat exhibited these pericellular spaces with remarkable distinctness (*vide* Plate 1. fig. 1). In this instance the distension of the lymph-sac was probably accompanied by a general plethora of the whole lymph-system of the brain, as the appearance of the nerve-cell would scarcely warrant the supposition of the appearance being entirely due to the shrinking of the cell consequent upon chrome hardening. The morbid conditions interfering with the perivascular lymph-current of the brain are numerous. Hyperæmia acts in this way and greatly modifies the nutritive and depurative changes proceeding in the pericellular sacs. Deposits within the perivascular sheaths, aneurismal dilatations along the vessels, tubercular outgrowths from their walls, proliferating connective elements, may all in their turn affect very materially the free exit of lymph from the perivascular channels, which is a *sine quâ non* for the maintenance of the functional activity of nerve-cells.

The proliferation of the connective elements of the neuroglia is represented in Plate 2. figs. 5 and 6, the latter being a fresh-teased preparation, while the former is from a section obtained by means of the freezing microtome. The truly protean forms assumed by these connective elements are well seen in such specimens, the nucleated corpuscle becoming swollen, cloudy, and often coarsely granular; at other times it maintains through all its changes of form the pellucid delicate appearance of its protoplasm.

Methods of Examination of the Cortex Cerebri.

It will be necessary for me to dwell shortly upon the methods employed by myself in this investigation. I have not limited myself to any conventional process for obtaining sections; but, whilst making free use of

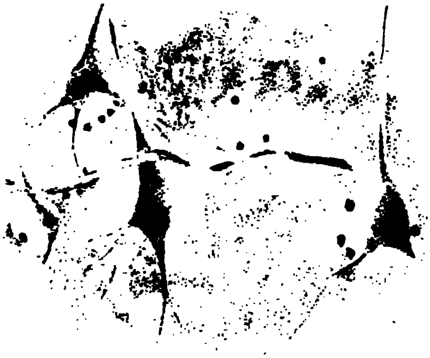


Fig. 1

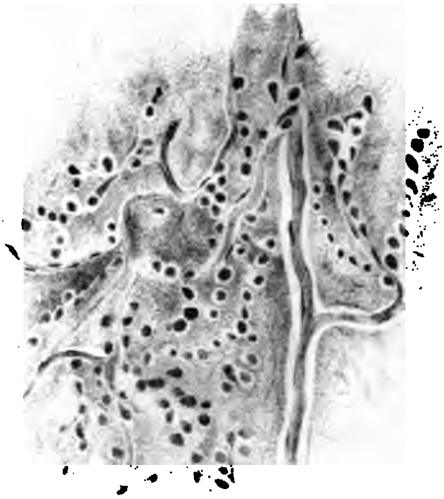


Fig. 2

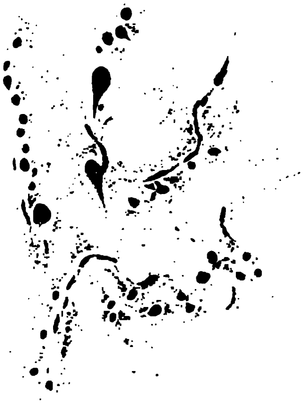


Fig. 3

ordinary pencil, which is polarized in the axial plane, at which both systems at once will be seen distinctly. And the ordinary index, which will be given by the ordinary image, will be a geometric mean between the two apparent extraordinary indices, of which one, namely, that got from the lines in the axial plane, will be the real extraordinary index.

There are two uniaxal crystals, calcite and quartz, for which we know accurately the principal refractive indices for the principal lines of the spectrum from the measures of Rudberg. The principal indices for these two minerals and the apparent indices in the two directions mentioned above are given in the following Table. The indices are given to four places of decimals, and the fixed lines C, D, E are chosen, whence the results applicable to the kinds of light most likely to be employed may be obtained, directly or by interpolation.

Lines.	Calcite.				Quartz.			
	μ.	μ'.	$\frac{\mu'^2}{\mu}$.	$\frac{\mu^2}{\mu'}$.	μ.	μ'.	$\frac{\mu'^2}{\mu}$.	$\frac{\mu^2}{\mu'}$.
C ...	1·6545	1·4846	1·3321	1·8438	1·5418	1·5509	1·5601	1·5328
D ...	1·6585	1·4864	1·3322	1·8505	1·5442	1·5533	1·5624	1·5352
E ...	1·6636	1·4887	1·3322	1·8590	1·5471	1·5563	1·5656	1·5380

It is well known that the double refraction of quartz differs from that of the generality of uniaxal crystals. Its wave-surface for any colour, instead of being the sphere and spheroid of Huyghens, is a surface of two distinct sheets, which, instead of touching, only make a very close approach along the axis. The polar diameters of the outer, or ordinary, and of the inner, or extraordinary, sheet differ by minute and practically equal quantities from the equatorial diameter of the ordinary sheet. The effect of this, however, on the indices, real or apparent, determined by Mr. Sorby's method on a plate cut perpendicular to the axis, would not be sensible. The peculiarity would show itself by giving the two images at different depths circularly polarized, one right-handedly and the other left-handedly.

It may be noticed that the refractive index is given by the reciprocal of the radius of curvature of a section of the wave-surface by a plane perpendicular to the lines seen in focus, and that in order that the lines may be seen distinctly, they must be perpendicular to one of the planes of principal curvature. This rule, as I proceed to show, is general; and it will much simplify the calculation in more complicated cases, by enabling us to dispense with the direct application of Huyghens's construction.

Let O be a point in the first surface of the plate, and consider a small

the ordinary preparation by chrome hardening, and the subsequent staining with logwood, carmine, and other dyes (adopting Lockhart Clarke's method for clearing and mounting), I have struck out independently a series of investigations devoted to the preparation of the brain in the *fresh state*, feeling convinced that the disuse of hardening agents would eliminate many sources of fallacy. A method was adopted by me and described at length in the 'Monthly Microscopical Journal' (Sept. 1876), whereby the cells and cortical structure generally could be well displayed. The great fault attached to this process is that relationships were wholly sacrificed for clearer definition of structure and differentiation of elements. Having endeavoured, without success, to obtain satisfactory results by the ordinary freezing methods with ice and salt, I eventually devised an instrument whereby freezing with ether spray was introduced, and all the conditions requisite for cutting the *finest sections* of *fresh brain* were obtained. This instrument was made for me by Mr. Baker of High Holborn, and is described by me in the 'Journal of Anatomy and Physiology' for April 1877, and proves to be of the greatest value in the investigation of nervous structures.

When perfectly fresh sections of healthy brain are thus obtained, and the slightest possible staining of the nerve-cells produced by a drop of carmine, or a ·25 per cent. solution of aniline black, just sufficing, in fact, to give us the outline of the cells, nuclei, nucleoli, and their environment, we are able to detect, in most cases, indications of spaces surrounding the cell; and especially is this the case in conditions which induce a shrinking of the cell-protoplasm, or, on the other hand, of the surrounding neuroglia.

The examination of these films of fresh cortex by chemical reagents, their examination after staining by aniline and mounting in balsam, the process of teasing above alluded to, as well as chrome hardening for sections, have been the methods employed in this investigation.

EXPLANATION OF THE PLATES.

Plate 1.

Fig. 1. Section obtained from the ascending frontal gyrus of a healthy young cat. Illustrative of the relationships of the pericellular lymph-sacs to the blood-vessel and its lymphatic sheath (the brain in this case was hardened and stained simultaneously). × 180 diameters.

Fig. 2. Section from the frontal lobe of a kitten at birth. Exhibits the young nerve-cells of the cortex within their lymphatic sacs arranged along the perivascular channels. The neuroglia is finely granular, the perivascular sheaths are distended, and the nerve-cells fail to exhibit clear indications of a nucleus. × 210 diameters.

Fig. 3. Similar appearances shown in a section obtained from the same convolution in a dog at birth. The nerve-cells are becoming elongated, pyriform, and in some cases exhibit a nucleus. × 210 diameters.

PLATE 2.

Fig. 4. A "giant cell" from the ascending frontal convolution in man. Obtained from *recent brain* by means of the new freezing microtome. × 105 diameters.

Fig. 5. Section from the ascending frontal convolution in a case of senile atrophy. Obtained, by means of the new freezing microtome, from fresh brain. The proliferation of connective cells of the upper cortical layers is seen invading the vascular tracts and nervous elements. × 180 diameters.

Fig. 6. A cluster of amœboid connective cells from the third layer of the ascending frontal convolution, in a case of senile atrophy. Section obtained from *fresh brain* by pressure and teasing.

III. "On the Thickness of Soap Films." By A. W. REINOLD, M.A., Professor of Physics in the Royal Naval College, Greenwich, and A. W. RÜCKER, M.A., Professor of Physics in the Yorkshire College of Science, Leeds. Communicated by R. B. CLIFTON, F.R.S., Professor of Experimental Philosophy in the University of Oxford. Received June 13, 1877.

Attempts have from time to time been made by various physicists to obtain from the phenomena of capillarity, or from observations on liquid films, an indication of the magnitude of the radius of molecular attraction. The authors of this note have, with the same object in view, lately made a series of experiments to determine whether the law that the resistance offered to the electric current by a uniformly thick homogenous body varies inversely as the section is or is not apparently obeyed by liquid films, as any apparent departure from that law might be taken to indicate a want of homogeneity, or that the thickness of the film was comparable with the magnitude of the radius of molecular attraction.

Their investigations on this point are not as yet sufficiently advanced for publication; but in the course of their work they have made some observations on the forms of soap films, which they venture to lay before the Royal Society in a preliminary note.

A liquid film, inclined to the horizontal so as to become gradually thinner by the slow descent of the liquid, will, under favourable conditions, appear black, as in the central portion of Newton's rings. By optical methods it is only possible to obtain a superior limit to the thickness of the black portion of such a film; but there is no doubt that at the lower boundary of the black the thickness of the film increases with extraordinary rapidity.

As a general rule no trace of the blue of the first order can be perceived; but when the colour next below the black is the white of the first order, the line of separation between the two is very definite; and if the film be moved so as to change the angle of incidence of the light by which it is viewed, the position of the boundary of the black remains fixed, though that of each of the other colours is altered with every motion. More frequently, however, the presence of the colours of the first order, and some or all of those of the second order, can only be detected by means of a microscope, and to the naked eye several tints appear to be wanting between the black and the colour which immediately succeeds it.

The constant recurrence of the phenomenon above mentioned, viz. the very rapid change in the thickness of the film in the immediate neighbourhood of the black, suggests an intimate connexion between the thickness of the latter and the molecular constitution of the liquid. Any investigation as to whether such a connexion exists must, it appears, be commenced by seeking the answers to the following questions:—

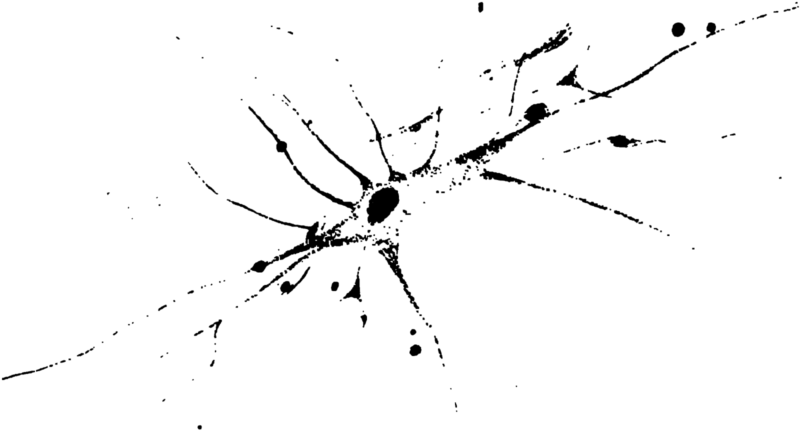
1. Do very rapid changes in thickness occur elsewhere in the films?
2. Is the black portion of the film uniform in thickness?
3. If so, is the thickness the same for all films formed with the same liquid?

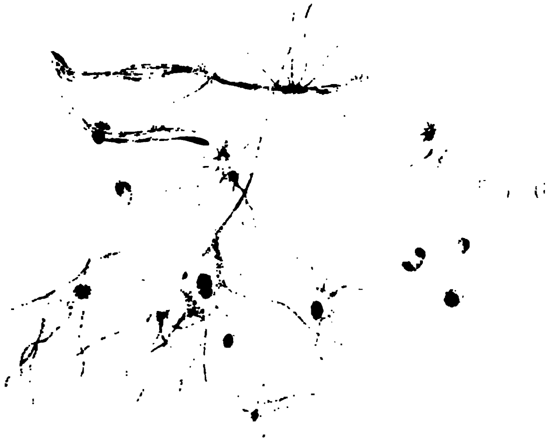
The authors have, with a single liquid and with the form of apparatus employed, obtained results which, although not sufficiently numerous to enable them to give general answers to these inquiries, display, as they believe, a hitherto unsuspected constancy in the phenomena exhibited by liquid films thinning under the influence of gravity, and seem to merit further study.

The method employed was to measure simultaneously the electrical resistance of the films, and the breadths of the bands of colour they displayed.

The liquid used was M. Plateau's "liquide glycérique," made by dissolving 1 part by weight of oleate of soda in 40 parts of water, and adding 3 volumes of this liquid to 2·2 volumes of Price's glycerine. To improve the conductivity, 3 parts by weight of potassium nitrate were dissolved in every 100 parts of water along with the oleate of soda.

The films which were submitted to investigation were cylindrical in shape, and were formed between two platinum rings of the same diameter placed one vertically over the other. The mode of supporting these and making the electrical connexions was as follows:—A glass cylindrical vessel, about 16 centims. high and 9 centims. in diameter, was fitted with an ebonite cover divided into two unequal parts. Each of these would, if placed on the top of the vessel, remain *in situ.* A brass tube, which could be elevated or depressed, and was retained in its position by friction, passed through the larger portion of the cover, and was, when in position, in the centre of the glass vessel. A piece of india-rubber tubing provided with a pinchcock was attached to the upper end of this tube, and to the bottom of it was soldered a brass plate carrying the upper platinum cylinder. This latter was formed of stout platinum foil, the edges of which were welded together in order to avoid the introduction of any





of normal sections perpendicular to the principal plane, we see that if b be the greatest or least parameter they remain constantly positive. But if b be the mean parameter, both expressions change sign twice, once in passing through zero, and once through infinity, as θ changes from 0° to 90°.

The radii of curvature become infinite together when

$$\tan^2\theta=\frac{a^2-b^2}{b^2-c^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

that is, when the plate is perpendicular to the optic axis. For a point in the circular section the radius vanishes when

$$\tan^2\theta=\frac{c^2(a^2-b^2)}{a^2(b^2-c^2)}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

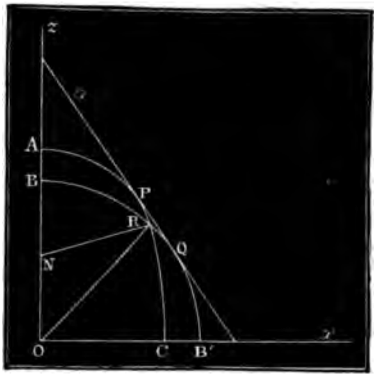
that is, when the plate is perpendicular to the ray-axis. For a point in the elliptic section the radius vanishes when

$$\tan^2\theta=\frac{a^2(a^2-b^2)}{c^2(b^2-c^2)}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

that is, when the plate is perpendicular to the normal to the elliptic section at the point where the two sections intersect.

A figure may make these changes clearer. Let xOz be a quadrant of

Fig. 3.



the plane perpendicular to the axis of mean parameter. Let BB′ be the circular, and AC the elliptic section, intersecting in R, PQ the common tangent, RN a normal at R to the elliptic section. Conceive a plate cut perpendicular to the plane of xz, its normal being inclined at the angle θ to Oz; and imagine θ to change continuously from 0 to 90°: and let ρ', ρ_1' represent the radii of curvature in the secondary plane (xOz being deemed the primary plane) for points in the sections AC, BB′ respectively. As θ starts from zero, ρ' starts from a and increases, and ρ_1' starts from $b^{-1}c^2$ and decreases. When θ becomes BOR, ρ_1' vanishes, and beyond that becomes negative, while ρ' continues to increase. As θ increases to BOQ, ρ' increases positively, and ρ_1' negatively, to infinity, and beyond that both change sign, ρ' becoming negative and ρ_1' positive. As θ increases to ANR, ρ' decreases negatively to zero, while ρ_1' decreases positively from infinity. On passing ANR, ρ' becomes positive, and increases to its final value, c, which it reaches when $\theta = 90°$, while ρ_1' decreases to its final value, $b^{-1}a^2$. Thus though $a > c$ we may say that as θ increases from 0 to 90°, ρ' *increases* from a to c by passing through ∞ and 0, and ρ_1' *decreases* from $b^{-1}c^2$ to $b^{-1}a^2$ by passing through 0 and ∞.

The extravagant changes of apparent index in the immediate neighbourhood of the wave- and ray-axes could probably not well be followed by the microscope, on account of the necessity of working with pencils of finite angular aperture, which would make the phenomena of focusing blend themselves with those of conical refraction. But there can be little doubt that a large increase or diminution of apparent index on approaching the critical region would be readily discernible. That these changes are not confined to the principal plane is evident, inasmuch as one principal radius of curvature of the wave-surface becomes infinite at any point of the circle of contact of the surface with the tangent plane perpendicular to the optic axis, and one principal radius of curvature vanishes at the conical point, to whatever normal section it be thought of as belonging.

Let us now resume the equations (2), (10), which give the principal curvatures for the elliptic section, without deciding beforehand any thing as to the relative magnitude of the parameters.

As θ changes from 0 to 90°, the radius of curvature in the primary plane changes from $a^{-1}c^2$ to $c^{-1}a^2$, and that in the secondary plane from a to c; and the ratio of the radii, therefore, changes from $a^{-2}c^2$ to $c^{-2}a^2$, of which one is greater than 1 and the other less than 1. If, then, both radii remain positive, as is the case in the two principal planes passing through the mean axis, the two radii must be equal for some intermediate value of θ. Hence there must be four umbilici in each of these planes. To find the umbilici we must equate the values of ρ, ρ' given by (2), (10), whence

$$a^2c^2\{(b^2-a^2)\cos^2\theta+(b^2-c^2)\sin^2\theta\}(\cos^2\theta+\sin^2\theta)$$
$$=\{a^2(b^2-a^2)\cos^2\theta+c^2(b^2-c^2)\sin^2\theta\}(a^2\cos^2\theta+c^2\sin^2\theta),$$

which gives, after reduction,

$$\tan^4\theta=\frac{a^2(b^2-a^2)}{c^2(b^2-c^2)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

This expression shows that the umbilici in the elliptic section made by the principal plane of greatest and least parameters are imaginary. If we take v_{xy}, v_{zy} to denote the inclinations to Oy of the normals to the umbilici in the planes of xy, zy, we have, by the requisite interchanges of letters:—

$$\tan^4 v_{xy}=\frac{a^2(a^2-c^2)}{b^2(b^2-c^2)};\quad \tan^4 v_{zy}=\frac{c^2(a^2-c^2)}{b^2(a^2-b^2)}. \quad . \quad . \quad . \quad (15)$$

If a plate be cut perpendicular to the normal at one of these umbilici, one of the polarized pencils which it transmits will give the images of both systems of cross lines distinct together; and the distinctness will not be affected by rotating the plate in its own plane while the cross lines are fixed. In this respect it agrees with a plate of a uniaxal crystal cut in an arbitrary direction, with which it might easily be confounded. But if the double refraction be strong enough to give a sensible *lateral* separation of the two oppositely polarized images, the two cases may be distinguished thereby in either of two ways:—First, if the images be compared with a mark fixed to the focus of the eyepiece, and the crystal be rotated in its own plane, while the object viewed through it is fixed, in the case of a uniaxal crystal the image free from astigmatism will remain fixed, while any point of the other describes a small circle round its mean position, whereas in a plate of a biaxal crystal cut perpendicular to the normal at one of the umbilici above considered it is the reverse; the image affected by astigmatism remains fixed, though its distinctness alters, while any point in the other describes a small circle about its mean position. Secondly, if the plane of separation of the two oppositely polarized images be noticed, in a uniaxal crystal the plane of polarization of the image which is free from astigmatism will be parallel to the plane of separation, while in a biaxal crystal cut as above supposed it will be perpendicular to the plane of separation*.

There are no umbilici in the circular sections of the wave-surface made by the principal planes. If we equate ρ' given by (7) to be the radius of curvature in the primary plane, we get, in fact, $\cos^2\theta+\sin^2\theta=0$, which cannot be satisfied.

The formulæ (15) give for v_{xy}, v_{zy} (ray D) in aragonite 69° 26′ and 45° 8′; in topaz 46° 49′ and 55° 27′.

In the employment of his method Mr. Sorby has chiefly had in view the discrimination of minerals, but it admits of one or two interesting applications to optical theory.

At the time when Fresnel invented his theory of double refraction it had been supposed, from the observations of those who had specially examined the question, that in biaxal crystals one of the rays obeyed the ordinary law of refraction; and Fresnel proved by two methods, both requiring skill on the part of the optician who cut the crystals, that the anticipation that his theory led him to entertain that that would not prove to be the case was verified. It is interesting to find that the extraordinary character of the refraction of both rays in a biaxal crystal admits of being established by such a comparatively simple mode of observation as that of Mr. Sorby.

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* [The first test supposes the surfaces of the plate to be pretty truly parallel, as otherwise it would produce displacement in consequence of its slightly wedge-shaped form; and Mr. Sorby thinks this requirement would prevent this test from being of much use. As to the second test, it is needless to observe that in any case in which we know independently which is the principal plane we need not attempt to observe the lateral separation of the images.—*October* 1877.]

The theory of Fresnel is confessedly wanting in rigour; and though the observations of Huyghens, of Wollaston, and of Malus proved that in Iceland spar Huyghens's construction, if not rigorously true, was at least a very close approximation to the truth, it seemed desirable to put it to a sharper observational test, more especially as different theories might lead to Huyghens's construction *as a near approximation*. For instance, in a paper read before the Cambridge Philosophical Society in 1849, I obtained a formula[*] which led me to perceive that double refraction would be simply accounted for by attributing it to a difference of inertia in different directions, such as would be produced if a fluid had to make its way among a number of bodies on the average regularly arranged, that arrangement being different in different directions, and that the wave-velocity on this theory would be related to the direction of the wave normal just as in the theory of Fresnel, with the exception that the reciprocals of wave-velocities would take the place of the velocities themselves. I refrained, however, from putting forward that theory either in the memoir referred to or elsewhere (though I have incidentally alluded to it in my report on double refraction[†]), because, on calculating the difference of refraction of the extraordinary ray on this theory and according to Huyghens's construction, at about 45° from the axis, where the difference would be greatest, I found it barely small enough, as seemed to me, to have escaped detection. Still this theory, which has occurred independently, in the same or a similar shape, to others[‡], led me to wish for a more exact verification; and in the report referred to I have proposed a method which seemed to me well calculated to lead to the desired result. This method I carried out some years later in the case of Iceland spar, though I did not publish the results; and I found that, to the limit of error of my observations (about 0·0001 in the index), Huyghens's construction was fully confirmed, while the error of the other was nearly a hundred times as great as the limit of error of the observations[§]. The accuracy of the Huyghenian law has also been confirmed by the elaborate observations of M. Abria*.

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[*] Cambridge Philosophical Transactions, vol. viii. p. 111.

[†] Report of the British Association for 1862, part i. p. 269.

[‡] See papers by the late Professor Rankin in the Philosophical Magazine, vol. i. (1851), p. 441, and by Lord Rayleigh in the same, vol. xli. (1871) p. 519.

[§] This result is briefly mentioned in the Proceedings of the Royal Society, vol. xx. p. 443.

In the method of prismatic refraction employed by M. Abria and myself, the difference between Huyghens's construction and the result of the theory just referred to is greatest about 45° from the axis, while extremely close to the axis, or to the equator, it would hardly be sensible. Mr. Sorby's method is remarkable for this, that it brings out into prominence variations of refraction with change of direction, though the absolute refractions which are involved may be nearly the same. Thus Mr. Sorby informs me that his method shows with perfect distinctness the two widely different foci for a plate of Iceland spar cut perpendicular to the axis, even though the inclination of the rays concerned to the axis is so small that, when polarized light is used, which is extinguished by an analyzer, the field remains dark after the interposition of the crystal.

In the theory referred to above the extraordinary sheet of the wave-surface is generated by the revolution of the curve which is the envelope of straight lines whose distance, v, from the origin is connected with their inclination, $90° - \theta$, to the axis by the relation

$$v^{-2} = a^{-2}\cos^2\theta + c^{-2}\sin^2\theta.$$

The radius of curvature of this envelope at the axis is $c^{-2}(2ac^2 - a^3)$, and accordingly the apparent index, m, is given by

$$m = \frac{\mu^3}{2\mu^2 - \mu'^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

which exceeds the apparent index, $\mu^{-1}\mu'^2$, given by the spheroid of Huyghens by

$$\frac{[(\mu^2 - \mu'^2)^2}{\mu(2\mu^2 - \mu'^2)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

The same formula will apply to a point in the equator if we interchange μ and μ'.

Putting these excesses into numbers, according to Rudberg's indices for the line D in Iceland spar, we find 0·0536 and 0·1180, giving for apparent indices 1·3858 instead of 1·3322 for a plate perpendicular to the axis, and 1·9685 instead of 1·8505 for a plate parallel to the axis when the microscope is focused on a line in the equatorial plane. These differences are much too large to escape detection.

POSTSCRIPT.

Being anxious to complete the theory of the images free from astigmatism by determining in what cases, if in any, they could be formed by transmission in a perpendicular direction across a crystalline plate cut otherwise than perpendicular to a principal plane, I have since worked out the differential equation (between two parameters) of the lines of curvature of the wave-surface, the discussion of which shows that there are no umbilici out of the principal planes. Hence the four directions determined by equations (15) are the only ones perpendicular to which if a plate be cut one of the images is free from astigmatism.—*October* 1877.

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* 'Annales de Chimie,' tom. i. (1874) p. 280.

XI. "Notes on Physical Geology.—No. III, On the probable Age of the Continent of Asia and Europe; and on the Absolute Measure of Geological Time." By Rev. S. HAUGHTON, M.D., D.C.L., F.R.S., Professor of Geology in the University of Dublin. Received June 11, 1877.

This paper was withdrawn by the Author after the reading, for correction of a numerical error, and will appear as amended next Session.

The Society then adjourned over the Long Vacation, to Thursday, November 15.

"On the Increase in Resistance to the Passage of an Electric Current produced on certain Wires by Stretching." By HERBERT TOMLINSON, B.A., Demonstrator of Natural Philosophy, King's College, London. Communicated by Prof. W. G. ADAMS, F.R.S. Received November 14, 1876. Read December 21 *.

The object of this iuquiry was (1) to determine the relation between increased resistance to the passage of an electric current and stretching-force; (2) to ascertain how much of the increased resistance in each case is produced by mere increase of length and diminution of section.

In order to determine the increase of resistance from stretching, the wires were each divided into two parts about 14 feet or more in length. One end of each part was fastened to a stout hook, firmly fixed into a block of wood, the two hooks used being about 8 inches apart, and the block of wood in which they were fixed securely fastened across two strong uprights, which were placed resting against the wall, so that the weights attached to the wires might swing clear of the table on which the uprights were placed. A loop was made at the other end of each part of the wire, and to this the weights were attached by means of strong hooks. The two parts of the wire were joined at the top, about 2 inches below each hook, by a piece of copper wire, which was securely soldered on to each part of the wire, so as to connect them; and toward the lower extremities of the two parts, about 5 inches above the points of attachment of the weights, two copper wires were soldered so as to connect the wires with a Wheatstone's bridge. The increase of resistance of the wires was measured by means of a sliding scale of platinum wire, divided into millimetre divisions, each equal to ·00166 ohm. As the object was to obtain the temporary and not the permanent increase of resistance (which permanent increase was found more or less with all the wires), weights slightly heavier than those intended to be used were first put on and taken off. Afterwards the wire was balanced as nearly as possible by German-silver wire, without the sliding scale, and then very exactly with the sliding scale, which was connected with one of two resistance-coils of 100 ohms each, which formed the other two sides of the bridge. The weights used were then carefully put on to the wires, and the increase of resistance measured by means of the sliding scale; the weights were next taken off again, and the sliding scale used for balancing once more. If there was any slight difference, as sometimes occurred, between the readings of the sliding scale before the weights were put on and after they were taken off, the mean of the two readings was taken.

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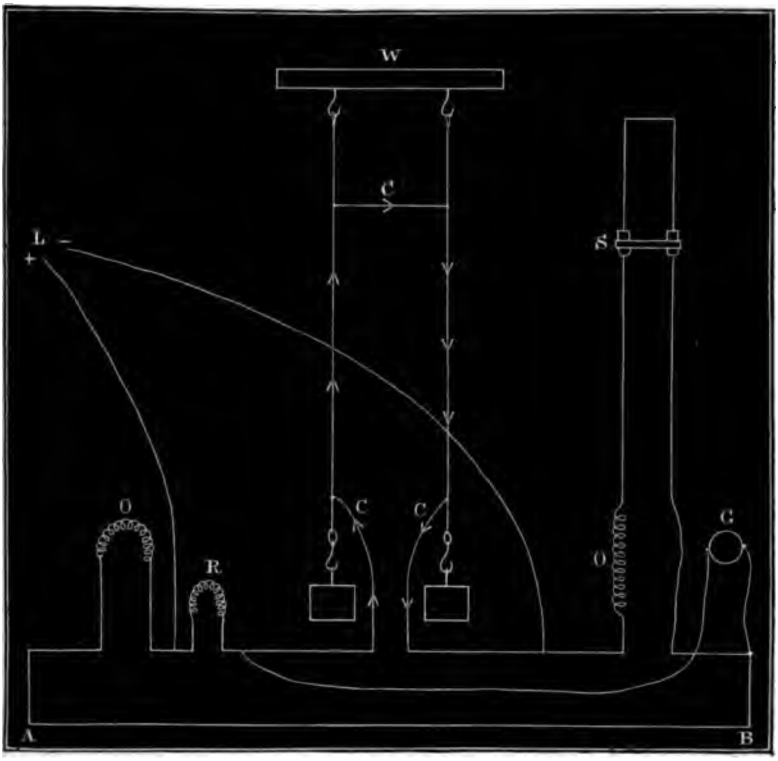
* See Proc. Roy. Soc. vol. xxv. p. 451.

In order to secure still greater accuracy, as many as eight or ten trials were frequently made with each particular weight, and the mean of all the trials taken. This precaution was necessary, as there were continual small variations from slight changes of temperature in the air of the room. These variations of temperature caused at first great trouble in the case of iron and steel wires, as the slightest difference of temperature was at once shown by the shifting of the light on the scale of the Thomson's reflecting galvanometer employed. This instrument was so delicate that the warmth of the hand placed 6 inches from the wire caused a perceptible shifting of the light. It was, indeed, partly owing to this difficulty that the observation of the permanent increase from stretching was not attempted. I have, however, since devised a plan for getting rid of this alteration from changes of temperature, and I hope to be able to make other experiments with wires in which the permanent as well as the temporary increase of resistance will be observed. Most of the observations of the steel and iron wires were made at first in the evening, with the doors and windows shut, as it was found almost impossible to take very exact observations in the daytime.

The disposition of the wires is shown in the accompanying sketch. In order to avoid heating the wires by the current, a single Leclanché cell was employed, and an increase of resistance $=\frac{1}{50000}$ of the whole could easily be measured.



W. Wooden block.
L. Leclanché cell.
O, O. Resistances of 100 ohms each.
R. German silver used to balance wire.
C, C, C. Connecting-wires of copper.
S. Sliding scale.
G. Galvanometer.

In this manner 4 pianoforte steel wires, 1 commercial steel wire, 3 iron wires, and 4 brass wires were examined with several different weights.

In the case of all the wires of different sections and materials, it was found that the increase of resistance was exactly proportional to the stretching-weight. In order to show this the increased resistance per lb. weight for the different weights employed is given in the following Table, for the first three steel wires examined, as with them a greater number of different weights were employed than with the other wires :—

	Number of pounds employed to stretch the wire.	Increase per pound, reckoned in divisions of the sliding scale.
Steel (No. 1). Section in square inches determined from loss of weight of a given length of the wire in water at 4° C. = 2231×10^{-7}. Total length of wire employed = 27·46 feet.	2	36·8
	3	36·8
	4	36·9
	5·75	36·8
	8·42	36·8
	9·75	37·0
	12	36·7
	12·5	36·4
		Mean.. 36·78
Steel (No. 2). Section in square inches, 6528×10^{-7}. Total length of wire employed = 27·5 feet.	2·75	12·72
	8	12·52
	16	12·52
	20	12·52
	24	12·52
	28	12·32
		Mean.. 12·52
Steel (No. 3). Section in square inches, 12563×10^{-7}. Total length of wire employed = 27·65 feet.	8	6·07
	12	6·08
	14	6·07
	20	6·05
	28	6·06
	40	6·08
		Mean.. 6·069

Though a smaller number of different weights were employed with the other wires, the proportion of the increase of resistance to the stretching-weight was quite as exact as in the case of the three examples given, never less than three different weights being used, and each of these tried several times.

The increase of resistance which a cubic centimetre of each wire would experience when stretched by a weight of 1 gramme was then calculated, and also the resistance of a cubic centimetre of each wire in ohms determined. The former values will be denoted by h, and the latter by the letter s.

The following Table gives the values of h, s, the section of each wire in square inches, and its specific gravity :—

	Sp. gr. (density of water at 18° C. taken as 1).	Section in square inches, determined from loss of weight in water at 4° C.	Increase of resistance of 1 cub. centim. for a stretching-force of 1 gramme, $=h$.	Resistance of 1 cub. centim. of the wire, $=s$.
Steel.			Ohms.	Ohms.
No. 1	7·8630	2231×10^{-7}	3009×10^{-17}	$1574{\cdot}8\times10^{-8}$
„ 2	7·8240	6528×10^{-7}	3159×10^{-17}	$1653{\cdot}1\times10^{-8}$
„ 3	7·7945	12563×10^{-7}	3445×10^{-17}	$1882{\cdot}4\times10^{-8}$
„ 4	7·8280	21009×10^{-7}	2982×10^{-17}	$1628{\cdot}7\times10^{-8}$
„ 5 (Commercial steel)....	7·7072	8477×10^{-7}	3511×10^{-17}	$1847{\cdot}0\times10^{-8}$
Iron.				
No. 1	7·7496	5003×10^{-7}	2557×10^{-17}	$1217{\cdot}6\times10^{-8}$
„ 2	7·5300	10239×10^{-7}	2637×10^{-17}	$1200{\cdot}8\times10^{-8}$
„ 3	7·6409	19010×10^{-7}	2712×10^{-17}	$1291{\cdot}0\times10^{-8}$
Brass.				
No. 1	8·4879	5781×10^{-7}	1843×10^{-17}	$656{\cdot}7\times10^{-8}$
„ 2	8·4984	8782×10^{-7}	1729×10^{-17}	$782{\cdot}2\times10^{-8}$
„ 3	8·4965	11327×10^{-7}	1565×10^{-17}	$744{\cdot}8\times10^{-8}$
„ 4	8·5048	18258×10^{-7}	1809×10^{-17}	$742{\cdot}9\times10^{-8}$

The values of h, divided by those of s, will give the increase of resistance for a stretching-weight of 1 gramme per unit of resistance. The following Table gives the values of $\frac{h}{s}$ for the different wires:—

Values of $\frac{h}{s}$, or increase of resistance per unit of resistance for a stretching-weight of 1 gramme on a cubic centim. of the material.

Steel.

(1)	$1910{\cdot}4\times10^{-12}$	Mean $=1875{\cdot}5\times10^{-12}$. Greatest departure from mean value less than 2·7 per cent.
(2)	$1910{\cdot}4\times10^{-12}$	
(3)	$1830{\cdot}5\times10^{-12}$	
(4)	$1825{\cdot}5\times10^{-12}$	
(5)	$1900{\cdot}7\times10^{-12}$	

Iron.

(1)	$2100{\cdot}0\times10^{-12}$	Mean $=2132{\cdot}2\times10^{-12}$. Greatest departure from mean about 3 per cent.
(2)	$2196{\cdot}6\times10^{-12}$	
(3)	$2100{\cdot}1\times10^{-12}$	

Brass.

(1)	$2229{\cdot}2 \times 10^{-12}$	Mean $= 2244{\cdot}9 \times 10^{-12}$. Greatest departure from the mean about 8·5 per cent.
(2)	$2211{\cdot}8 \times 10^{-12}$	
(3)	$2101{\cdot}5 \times 10^{-12}$	
(4)	$2435{\cdot}0 \times 10^{-12}$	

It will be seen from the last Table that the values of $\frac{h}{s}$ are as constant as could be expected in the case of wires of the same material, but differ for wires of different materials, being greatest for brass.

The value of Young's modulus for some of the wires was ascertained in the usual manner with the cathetometer. The wires were firmly secured at the top of a staircase, and a mark placed about 3 feet above the point of attachment of the weights at the bottom, so that the stretching of about 21 feet of the wire could be observed and the increase of length for different weights observed. A mark was also made near the top of the wire, in order to examine if there was any yielding of the support to which the upper end of the wire was attached, and for some of the wires a reading was taken both at the top and bottom marks; but as after several trials there was no apparent yielding, in the last experiments made the mark only at the bottom was observed. Weights heavier than those intended to be used were first put on and then taken off again several times; finally a weight was left on the wire sufficient to keep it perfectly straight. Then weights were carefully added, the increase of length observed with the cathetometer; and the weights having been taken off, another reading taken, and if there was any slight difference in the readings before adding the weight and after taking it off, the mean of the two readings was taken. The results agreed very fairly with each other—how much so may be gathered from the following example (two trials with each weight were taken):—

Iron (No. 2).

For a stretching-weight of	A lengthening was observed of
10220 grammes	·0005774 m.m. per gramme.
7710 „	·0005837 „ „
17930 „	·0005828 „ „
Mean	·0005813 „ „

The result may be taken as a fair average of the values obtained with the other wires.

The value of Young's modulus was also ascertained by means of longitudinal vibrations of sound. The two ends of the wire to be examined were securely soldered into two strong flat iron bars, one of the bars was fastened in a vice, firmly secured, whilst the other bar was held in the hand, and the wire pulled with sufficient force to enable a clear note to be got out of the wire by rubbing it longitudinally with a resined glove. The wire was then clipped in the centre and rubbed again to test the security of the fastenings, as it was found that if the bar in the vice was not well secured the note produced by clipping in the centre was not the octave of the note given out by the unclipped wire, but if the bar were well fastened a clear and perfect octave could be produced.

The number of longitudinal vibrations was ascertained by means of the siren, generally three or four trials of 3 minutes each being taken. In this work I was assisted by Mr. Furse (the assistant to the Professor of Natural Philosophy at King's College), to whom I am much indebted for help in my experiments. The records of the siren were very good indeed, in every case there never being a greater difference than $\frac{2}{10}$ per cent., and in many cases less. But on experimenting with different lengths of the wires the values of the modulus would vary as much as 1 or 2 per cent., so that, though some of the values obtained are recorded, I have chosen the more reliable ones of the cathetometer for calculating the increase of length of each wire when stretched.

In the following Table the amount of lengthening which a cubic centimetre of each wire experiences for a stretching-force of 1 gramme is given; these values will be denoted by $\frac{1}{e}$:—

	Values of $\frac{1}{e}$ obtained from observations with the cathetometer.	Values of $\frac{1}{e}$ obtained from observations on longitudinal vibrations.
Steel.		
(1)	—*	4866×10^{-13}
(2)	5279×10^{-13}	4928×10^{-13}
(3)	5082×10^{-13}	5073×10^{-13}
(4)	—*	
(5)	5665×10^{-13}	
Iron.		
(1)	4896×10^{-13}	
(2)	5938×10^{-13}	
(3)	5435×10^{-13}	
Brass.		
(1)	10120×10^{-13}	

In the case of those wires marked * there was not sufficient length to take good observations with the cathetometer.

Again dividing the values of $\frac{h}{s}$ by those of $\frac{1}{e}$ we shall obtain the increase of resistance per unit of resistance per unit increase of length.

Values of $\frac{h}{s} \div \frac{1}{e}$.

Steel.

(2)	3·619	Mean 3·525
(3)	3·602	
(5)	3·355	

Iron.

(1)	4·289	Mean 3·951
(2)	3·699	
(3)	3·864	

Brass.

(1)	2·203

It would seem that the increase per cent. of resistance for a given lengthening of a wire is greater in iron than in steel, and much greater in both iron and steel than in brass, but that the increase per cent. of resistance per unit of stretching-force employed is greater in brass than in iron, and greater in iron than in steel.

The torsional rigidity of the wires was then ascertained, the vibrators used being similar to those used by Sir William Thomson in his experiments on the rigidity and viscosity of metals (Proc. of Roy. Soc., May 1865), namely, thin cylinders of sheet brass, turned true outside and inside, supported by a thin, flat, rectangular bar. The wire to be tested passed perpendicularly through a hole in the middle of the bar, and was there firmly soldered. The cylinder was tied to the horizontal bar by light threads, so as to hang with its axis vertical; the other end of the wire was securely soldered into a stout iron bar, firmly held in a vice attached to a rigid support. Two experiments with different lengths of wire were made with each wire, the result of each of the two experiments agreeing very closely, as, for instance, we may take as an average example Steel No. 3, in which the torsional rigidities in grammes per square centimetre in the two trials were $783·5 \times 10^6$ and $782·2 \times 10^6$.

If we assume the wires to be isotropic, we can, from the values of e and the rigidity, which latter value will be denoted by r, obtain the ratio of lateral lineal contraction to longitudinal dilatation; denoting this ratio by σ, we shall obtain (Thomson and Tait's Nat. Phil. p. 521) $\sigma = \frac{e}{2r} - 1$, and therefore easily deduce the increase of resistance that would follow in the case of each wire from mere increase of length and diminution of section, without considering any other alteration of resistance that would result from stretching. Calling ds the increase of resistance that would result from mere increase of length and diminution of section, we can prove that

$$\frac{ds}{s} = \frac{1}{r} \quad \frac{1}{e}.$$

that we are not in a position to form any conclusion as to their size, excepting that they are so small as to be beyond the reach of observation. Dr. Tyndall has taught us, first, that the optical effects observed when a beam of light passes through a particulate atmosphere are such as could only be produced by light-scattering particles of extreme minuteness; and, secondly, that by subsidence these particles disappear, and that the contaminating property of the atmosphere disappears with them. He has thus approximately determined for us the upper limit of magnitude, but leaves us uncertain as to the lower; for we have no evidence that the particles which render the atmosphere opalescent to the beam of the electric lamp may not be many times larger than those which render it germinative. Consequently the fact that the air may be rendered sterile by subsidence, while affording the most conclusive proof that germinal matter is not gaseous, leaves us without information as to the size of the particles of which it consists.

Of each germinal particle, whether inhabiting an aqueous liquid or suspended in the atmosphere, it can be asserted that under conditions which occur so frequently that they may be spoken of as general (viz. moisture, a suitable temperature, and the presence of dead proteid matter, otherwise called organic impurity) it produces an organism. If, for the sake of clearness, we call the particle *a* and the organism to which it gives rise A, then what is known about the matter amounts to no more than this, that the existence of A was preceded by the existence of *a*. With respect to A, we know, by direct observation, that it is an organic structure; but inasmuch as we know absolutely nothing as to the size and form of *a*, we cannot even state that it is transformed into A, much less can we say any thing as to the process of transformation.

Considering that it is admitted on all hands that there exist in ordinary air particles which are potentially germs, it might at first sight appear needless to inquire whether or not this fact is to be regarded as carrying with it the admission that they must necessarily possess the other attributes of organized structure. Very little consideration, however, is requisite in order to become convinced that this question stands in relation with another of fundamental importance in biology—that, namely, of the molecular structure of living material *. It is not necessary for my present purpose to do more than to indicate the nature of this relation. As regards every form of living matter, it may be stated that quite irrespectively of its morphological characteristics, which, as we have seen, must be learnt by the application of the various methods of visual observation at our disposal, it possesses molecular structure peculiar to itself. We are certain of this because the chemical processes of which life is made up are peculiar, that is, such as occur only in connexion with increase of resistance from stretching, when the weights were taken off the wire did not immediately attain the resistance which it ultimately settled at, but gradually recovered itself in about three minutes.

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* The reader who is interested in this subject will find it discussed with great ingenuity by Prof. Pflüger, in his paper "Ueber die physiologische Verbrennung in den lebendigen Organismen," Pflüger's Archiv, vol. x. p. 300.

This effect was very perceptible with the thicker wires, where heavy weights were employed. I suppose the effect may, at any rate, be partly attributed to the heat which would be generated when the wire recovered its former volume; but I am not at all satisfied that the whole of the effect observed was due to this cause.

The conclusions to be drawn from the experiments are:—

1. That the temporary increase of resistance of a wire when stretched in the same direction as the current is exactly proportional to the stretching-force.

2. That the increase per cent. of resistance when a cube of each material is stretched by the same weight is greater in iron wire than in steel wire, and greater in brass wire than in iron wire; also that this increase is nearly the same for different specimens of the same material.

3. That the increase per cent., when the material is stretched to the same extent, is much greater in iron and steel wires than in brass wire, and is probably greater in iron wire than in steel wire.

4. That there is a residual increase in each case, over and above that which would follow from mere increase of length and diminution of section, that this residual increase is much greater in iron and steel than in brass, and greater in iron than in steel.

November 15, 1877.

Sir JOSEPH HOOKER, C.B., President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Mr. Henry Nottidge Moseley was admitted into the Society.

Mr. Abel, Prof. Carey Foster, Mr. Huggins, Prof. Jevons, and Prof. Parker, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the table, and thanks ordered for them.

A portrait in gilt frame of Sir John Herschel, by the Danish artist Jensen, presented by Mr. John Evans, F.R.S., and a portrait in gilt frame of Albert von Haller, by Van Stoppelaer, presented by Dr. Sharpey, F.R.S., were laid before the Meeting; and after the President had read the following letter, a special vote of thanks was given to the donors of the paintings:—

Nov. 14, 1877.

Dear Mr. President,—I take leave to offer for the acceptance of the Royal Society the accompanying Portrait, painted, as appears from an inscription on the back, by C. van Stoppelaer in 1765, and believed to be a likeness of Haller. I regret I am unable to give any history of the picture, except that M. Michel, a foreign gentleman from whom I acquired it, informed me that it had been for many years in the possession of his wife's family, and was reputed to be a portrait of Haller. The date 1765 would correspond with the 57th year of Haller's life.

I remain,
Yours faithfully,
W. Sharpey.

Sir J. D. Hooker, P.R.S.

The Bakerian Lecture—"On the Organization of the Fossil Plants of the Coal-measures.—Part IX. On the latest Researches into the Organization of the Fossil Plants of the British Coal-measures, especially of the *Calamites* and *Lepidodendra*"—was then delivered by W. C. Williamson, F.R.S., Professor of Natural History, Owens College, Manchester. The following is an abstract:—

The first plant noticed is one of which a transverse section was figured in the lecturer's Memoir, Part I., under the belief that it was Calamitean. It now proves not to be so, but is a branching non-articulated plant, lacking the nodes and the longitudinal internodal canals so characteristic of the Calamites. It has a large parenchymatous medulla with radiating prolongations separating the very distinctly defined wedges of the vascular zone. From the peculiar shape thus given to transverse sections of the medulla the author has assigned to the plant the provisional name of *Astromyelon.* In the place of the canal of Calamites the thin medullary extremity of each vascular wedge is occupied by a few larger and often more conspicuous vessels than those forming the rest of the wedge. The medulla further differs from that of Calamites in being rarely fistular. Each wedge consists of a series of regular radiating laminæ of barred vessels separated by numerous medullary rays—the latter varying in composition from a solitary cell to numerous cells arranged in single vertical series. Nearly all the stems and branches of this plant are found to be decorticated. One specimen found by Mr. Butterworth is surrounded by a very thin cortex consisting only of three or four layers of parenchymatous cells. *Astromyelon* forms another example of the numerous carboniferous plants whose vascular zone grew by exogenous additions, yet the exclusion of every modification of tissue, except barred vessels, from the vascular laminæ suggests Cryptogamic rather than Gymnospermous affinities.

Astromyelon is the only distinct TYPE of plant left undescribed in the lecturer's previous Memoirs; but he has obtained a considerable amount of additional information respecting some of those previously examined.

Calamites.—A description is given of a series of specimens, beginning with very small twigs composed of parenchyma, which is only divided into pith and bark by a ring of the internodal canals characteristic of Calamites. The medulla is continuous and not fistular. Then follow others in which very slight traces of vascular tissue are seen external to each internodal canal, and which constitute the beginnings of a corresponding number of vascular wedges; still higher in the series of sections others are arrived at in which these vascular wedges are more fully developed; the medulla has become fistular and the cortical cells are differentiated into a coarser tissue than the medullary one. A Calamite of much larger dimensions is then described, in which the fistular medulla is enclosed within a large vascular cylinder, the component radiating laminæ of which are fully two inches in length. Sections of this example show that the peculiar Calamitean features seen both in transverse and tangential sections of all young specimens, and which features are also very distinctly seen at the inner portion of the vascular zone of the specimen described, almost wholly disappear towards its periphery. The bark of this example has undergone marked development and differentiation. There is an inner zone of parenchyma, passing quickly into a thick middle one of prismatic cells, identical with the "couche subéreuse" of M. Renault, or corky layer seen in *Lepidodendron* and *Sigillaria*. This has obviously not been the outermost layer of the bark; the specimen had been weather-worn. A series of additional facts are given, illustrating the nature of what the lecturer has previously termed the infranodal canals, and also the structure and arrangement of the branches given off from the nodes. He shows that the young branches do not exhibit all the peculiar Calamitean conformation until they have emerged from the vascular zone of the parent stem; whilst older ones develop a thick vascular zone, like, and continuous with, that of the parent stem. Hence he concludes that the minute specimens already referred to in which the internodal canals exist, but have few or no vascular bundles associated with them, are probably not branches, but young plants developed directly from spores.

The lecturer again points out the extreme improbability that two genera belonging to types so widely apart from each other as the Gymnospermous Exogens and the Cryptogamous Equisetaceæ should exhibit such minute organic resemblances as obviously exist between the supposititiously separate genera *Calamodendron* and *Calamites*. Further it is to be remembered that, notwithstanding the extreme abundance of the supposed equisetiform Calamites in the shales of the Coal-measures, no one has yet discovered a single fragment retaining its internal organization that does not prove to be a so-called *Calamodendron*. He shows that the organization of the latter specimens explains the minutest details of the common impressions and casts found in the shales and sandstones of the Coal-measures; and hence he continues to hold his previous opinion that the separation of the Carboniferous Calamites into two types is a fundamental error, and unsupported by any justifying facts.

M. Grand'Eury having disputed the lecturer's conclusions respecting the structure of *Asterophyllites*, under the supposition that *Sphenophylla* have been mistaken for examples of that genus, an additional example is published, demonstrating that the long linear leaves were uninerved, and that consequently the specimens could not possibly have been *Sphenophylla*, the essential characteristic of that genus being that it is multinerved.

Lepidodendron and Sigillaria.—The lecturer notices the continued separation of these two genera by M. Grand'Eury and some other observers, and especially the unintentional misrepresentation by M. Grand'Eury of the facts which lead the lecturer to recognize a very close relationship between them. The only distinction which is supposed to exist, even by the most earnest advocates, for making *Lepidodendron* a Cryptogam and *Sigillaria* a flowering Gymnosperm, is the presence, in *Sigillaria*, of an outer exogenous vascular zone, in addition to the inner non-exogenous one seen in all the *Lepidodendra*. M. Grand'Eury admits, in the most definite language, that in all other features these groups of plants possess exactly the same organization. The lecturer recalls attention to the fact that in the Burntisland *Lepidodendron*, supposed to be *L. Veltheimianum*, the exogenous zone is absent from the young twigs, but is gradually developed in the twigs as they expand into branches; hence, according to the Brongniartian hypothesis, the plant is a *Lepidodendron* in its young state, and a *Sigillaria* in its matured condition. Additional illustrations are given from the stems of *Lepidodendron selaginoides*, the species in which the exogenous zone is the least developed of any of the plants which possess such a zone, but which has no claim whatever, beyond its possession of an exogenous cylinder, to be regarded as a *Sigillaria*. The minute details of the structure of that zone, though feebly represented, exhibit, nevertheless, every essential characteristic of the same zone in Sigillarian stems. These and other similar facts lead the lecturer to adhere to his previous conclusions, and to regard the *Sigillariæ* as Cryptogams, representing only the highest forms of the Lepidodendroid type of vegetation—conclusions to which some more recent investigations of the Arran fossils, to be published in a future memoir, give the clearest support.

A very remarkable series of Lepidodendroid spores have been discovered in the Halifax beds by Mr. Spencer and Mr. Binns. The most abundant of these are microspores of the usual type associated with numerous macrospores of a new form. These are round bodies with a curious projecting organ on one side surrounded by a thickened ring of the spore wall, reminding the observer of the projection, with its thickened surrounding ring, seen at the apex of the macrospores of *Isoëtes lacustris.* These macrospores have their exteriors irregularly clothed with numerous long radiating hair-like fringes, some of which are simple, others branched twice or three times, whilst in the interior of some of them the endospermic cells are beautifully preserved. Along with these Messrs. Binns and Spencer have discovered a new and very remarkable *Lepidostrobus.* Its almost spherical sporangia contain numerous spores arranged in tetrasporal clusters, each cluster being imbedded in a larger group of mother and their sister cells, which are exquisitely preserved. Each united cluster of four spores is about $\frac{1}{120}$ of an inch in diameter. Whether these objects are a new and very large species of microspore—macrospores clustered in unusual numbers within the same sporangium—or whether they are merely mother cells from which at a later period clusters of microspores would be developed, is doubtful. Some of these tetraspores enclose a number of minute cells.

Along with the above other smaller forms of what appear to be macrospores have been found. Their exterior is adorned with short trifid appendages, calling to memory the fossil *Xanthidia* of flint and the zygospores of some of the Desmidieæ. Some remarkable sporiferous (?) cavities imbedded in parenchyma significantly recall the imbedded sporangia of *Ophioglossum vulgatum*, whilst a very singular section of a rather large hollow spherical body, composed of a single layer of hourglass-shaped cells arranged vertically to its surface, and many of which cells are prolonged into external hairs, bears no inconsiderable resemblance to a section of a sporangiocarp of *Isoëtes lacustris.*

Ferns.—Two types of stem or petiole, not hitherto met with in Yorkshire or Lancashire, have been found recently. One of these is from Oldham, whence it was obtained by Mr. Isaac Earnshaw. It is a petiole differing so little from the *Chorionopteris Gleichæ* of Corda that the lecturer has included it in his provisional genus *Rachiopteris* as *R. Gleichæ.*

The affinities of the second plant are more doubtful. It is a cylindrical axis from Halifax, where it was found by Messrs. Spencer and Binns, to both of whom the lecturer is indebted for specimens of it. Its most conspicuous feature is its very large central vascular bundle, which is a cylindrical mass of barred vessels. The bundle is of much larger dimensions, proportionate to the size of the stem, than in any Fern or Lycopodiaceous plant that has hitherto been found in the Coal-measures. It gives off branches almost at right angles to the main axis, and these are met with of all sizes, from ·066 of an inch down to a mere speck. In some respects this plant has a Lycopodiaceous aspect, especially in the size and appearance of its central bundle. The bark, too, consists of an inner layer of delicate parenchyma and an outer one of coarser cells. These two are Lepidodendroid features. The provisional name of *Rachiopteris cylindrica* is assigned to it.

Two fragments have been met with that correspond very closely with some figured by M. Grand'Eury as belonging to *Cordaites.* One of these is a fragment of epidermis densely crowded with stomata; the other is part of the transverse section of a leaf exhibiting a single row of vascular bundles.

The Arran beds have furnished a very striking fragment of wood to which the lecturer gives the name of *Lyginodendron anomalum*, and which appears to exhibit, in a most exaggerated form, the peculiarities of the vascular axis of the *Lyginodendron Oldhamium* previously described. Its remarkable characteristic is the enormous size of its medullary rays, which, in tangential sections of the wood, have a lenticular shape—a contour which causes the thin vascular laminæ, when seen in such sections, to form a perfect network, the large meshes of which are occupied by the cells of the medullary rays.

The Oldham nodules frequently furnish fragments of the bark of some large trees. One of the most perfect of these is described, and consists of an inner parenchyma, radiating into a very thick layer of prismatic parenchyma, and an outer one of the more common type of parenchyma. Intermediate between these two latter is a very remarkable zone of meristem tissue where active genetic action has obviously gone on during the life of the plant. There thus appear to have been two parallel cylindrical zones of active growth in this bark—one between the vascular cylinder and the innermost bark, and the other between the prismatic tissue or modified bast layer and the subepidermal parenchyma. At the junction of the two latter tissues the tangential sections show that the prismatic cells are grouped in undulating vertical laminæ, forming a network, the meshes of which are occupied by tabular parenchymatous cells which stand on their edges with their flat planes arranged tangentially to the stem. These cells indicate an extremely active state of cell-multiplication. There is every probability that this type of bark is Sigillarian.

Some curious little circular disks, surrounded by a ring of seven or eight yet smaller disks, are described: they are from Halifax. Their nature being problematical, the provisional name of *Oidospora anomala* is assigned to them. The lecturer concludes by calling attention to the remarkable prevalence of prismatic parenchymatous cells in *Calamites*, *Lepidodendron*, *Sigillaria*, and *Asterophyllites*, in the medullary rays of Calamites, and especially in the middle bark of all these plants, where it has constituted a modified form of phellem or corky tissue.

November 22, 1877.

Sir JOSEPH HOOKER, C.B., President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—Sir Joseph Dalton Hooker, C.B., M.D., D.C.L., LL.D.

Treasurer.—William Spottiswoode, M.A., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Thomas Henry Huxley, LL.D.

Foreign Secretary.—Professor Alexander William Williamson, Ph.D.

Other Members of the Council.—Frederick A. Abel, C.B., V.P.C.S.; William Bowman, F.R.C.S.; Frederick J. Bramwell, M.I.C.E.; William B. Carpenter, C.B., M.D., D.C.L.; William Carruthers, F.L.S.; William Crookes, V.P.C.S.; Prof. P. Martin Duncan, M.B., P.G.S.; William Farr, M.D., D.C.L.; Prof. William H. Flower, F.R.C.S.; Prof. G. Carey Foster, B.A., F.C.S.; John Russell Hind, F.R.A.S.; Lord Rayleigh, M.A.; Vice-Admiral Sir G. H. Richards, C.B.; Prof. Henry J. Stephen Smith, M.A.; Prof. Balfour Stewart, M.A., LL.D.; Prof. Allen Thomson, M.D., F.R.S.E.

The Presents received were laid on the table, and thanks ordered for them.

Mr. Brian Haughton Hodgson was admitted into the Society.

The following Paper was read:—

"Remarks on the Attributes of the Germinal Particles of *Bacteria*, in Reply to Prof. Tyndall." By J. Burdon Sanderson, M.D., LL.D., F.R.S. Received September 17, 1877.

In a short paper communicated to the Royal Society at the close of last Session Prof. Tyndall did me the honour to criticize certain words reported to have been used by me at a meeting of the Association of Medical Officers of Health in January last. Although I am much indebted to him for the opportunity he has thus afforded me of discussing an important subject before this Society, I cannot refrain from expressing my regret that he should have thought it desirable to quote at length, and thus to place on permanent record in the Society's Proceedings, the expressions used on the occasion above mentioned. I regret this because these expressions occur in an abbreviated and incomplete abstract of a hastily prepared discourse not intended for publication.

As, however, I am well aware that Prof. Tyndall's purpose in his communication was not to criticize the language, but the erroneous views which the language appeared to him to contain, I shall make no further reference to the quotation, but shall regard it as the purpose of the present paper, first, to reply to the reasoning embodied in his last communication, and, secondly, to corroborate certain statements previously made by me, to which he has taken exception in the more extended memoir published in the 166th volume of the 'Philosophical Transactions.'

It will be my first object to enable the Fellows of the Royal Society to judge how far the views I entertain differ from those which have been enunciated here and elsewhere by Prof. Tyndall. Biologists are much indebted to him for the new and accurately observed facts with which he has enlarged the basis of our knowledge, as well as for the admirable methods of research with which he has made us acquainted. As regards the general bearing of these facts on the doctrine of Abiogenesis, I imagine that we are entirely agreed. So far as I can make out, the difference between us relates chiefly to two subjects, namely, the sense in which I have employed the words "germ" and "structure," and the extent of the knowledge at present possessed by physiologists as to the structure and attributes of the germinal particles of *Bacteria.*

Although Dr. Tyndall, in the title of his paper, refers to my "views of ferment," yet as he makes no further allusion to them, I will content myself with stating that in the passage quoted the first sentence (from the words "In defining" to the word "living") has nothing to do with the following sentences, having been placed in the position which it occupies in the quotation by the abstractor. The paragraph ought to begin with the words "Ten years ago."

Of the meaning which attached itself to the word "germ" in the days of Panspermism a correct idea may be formed from the following passage from M. Pasteur's well-known memoir "Sur les Corpuscules Organisés qui existent dans l'Atmosphère":—"There exist," says he, "in the air a variable number of corpuscles, of which the form and structure indicate that they are organized. Their dimensions increase from extremely small diameters to one hundredth of a millim., 1·5 hundredth of a millim., or even more. Some are spherical, others ovoid. They have more or less marked contours. Many are translucent, but others are opaque, with granulations in their interior . . . I do not think it possible to affirm of one of these corpuscles that it is a spore, still less that it is the spore of a particular species of microphyte, or of another that it is an egg or the egg of a particular microzoon. I confine myself to the declaration that the corpuscles are evidently organized; that they resemble in every respect the germs of the lower organisms, and differ from each other so much in volume and structure that they unquestionably belong to very numerous species." Such are the "germs" of M. Pasteur, and such is the conception of a germ which was entertained by informed persons up to 1870, and is very generally entertained up to the present moment*. It is obvious that these "corpuscules organisés" were, if they had any relation to the *Bacteria*, not germs in Dr. Tyndall's sense, but "finished organisms;" and yet it was of these that M. Pasteur said that it was "mathematically proved" that they were the originators of the organisms which are developed in albuminous liquids containing sugar when exposed to the atmosphere.

With reference to the word "structure" I would point out that in the passage quoted from my lecture it is distinctly stated that the bacterial germ is endowed with structure in the molecular sense, but not in the anatomical sense. The meaning of the expression "anatomical structure" was, naturally, not defined, considering that the persons whom I was addressing might be supposed to be familiar with it. As, however, my failing to do so has apparently led to some uncertainty as to my meaning, I must, to avoid future misunderstandings, define more completely the difference between the two senses in which the word was used by me.

The anatomical sense of the word structure may be illustrated by referring to its synonyms, to the English words texture and tissue, to the Greek word *ιστίον*, and to the German word *Gewebe*, from which two last the words in common use to designate the science of structure, viz. histology and *Gewebelehre*, are made up. What I have asserted of the germinal particles of *Bacteria* is, that no evidence exists of their being endowed with that particular texture which forms the subject of the science of histology. In biological language there is a close relation between the words structure and organization, the one being an anatomical, the other a physiological term; either of these words signifies that an object to which it is applied consists of parts or structural elements, each of which is, or may be, an object of observation. As the observation is unaided or aided, the structure is said to be macroscopical or microscopical. The biologist cannot recognize ultra-microscopical structure or organization except as matter of inference from observation, *i. e.* from observing either that other organisms, which there is reason to regard as similar to the object in respect of which structure is inferred, actually possess visible structure, or that the object can be seen to possess structure at a later period of its existence. As instances in which the existence of structure is inferred the following may be mentioned:—The protoplasm of a Rhizopod is admitted to have structure because, although none can be seen in the protoplasm itself, the complicated form of the calcareous shell which the protoplasm makes or models can be seen. By analogy, therefore, other organisms which are allied to the Rhizopod are inferred to have structure; and from these, or from similar cases, the inference is extended to all kinds of cells, with respect to which it is taught by physiologists that although, in certain cases, no parts can be distinguished, the living material of which they consist is nevertheless endowed with structure or organization. Similarly we assume that a *Bacterium* possesses a more complicated structure than we can actually observe, because in other organisms which are allied to it by form and life history such complications can be seen. Again, in all embryonal organs we admit the existence of structure before it can be seen, because in the course of development we observe its gradual emergence. So far inference of the existence of structure from historical evidence is justifiable; but if we were to carry this inference back to the ovum itself, and say that the characteristic structures of nerve, of muscle, or of gland exist in the ovum at the moment after impregnation, every physiologist would feel the assertion to be absurd.

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* Before I became aware that the contaminating particles of water are ultra-microscopical I myself was engaged earnestly in hunting for germs both in water and air. The search has been continued by others up to a much later period. Those who desire information on the organized particles of the atmosphere will find the subject exhaustively treated by Dr. Douglas Cunningham in a Report entitled "Microscopical Examinations of Air," lately issued by H.M. Indian Government.

In the familiar comparison of the origin of the elephant with that of the mouse, in which the perfect anatomical similarity of the ova in the two species is contrasted with the enormous difference of the result, we should be justified in saying that the difference of development is the expression of structural difference between the primordium of the one and the primordium of the other; but inasmuch as it is not possible to indicate any anatomical distinction, it is understood that structural difference of another kind is meant, namely, difference of molecular constitution. In other words, we assume that the potential difference between the one and the other is dependent on an actual difference of molecular structure. Whether this is accompanied with an anatomical difference, such as we might expect to be able to see if we had more perfect instruments, we do not know.

From the moment that it is understood that the word structure means anatomical structure, the argument used by Dr. Tyndall loses its relevance. After referring to the "germ-limit," he says, "Some of those particles" (by which, I presume, is meant atmospheric particles) "develop into globular *Bacteria*, some into rod-shaped *Bacteria*, some into long flexile filaments, some into impetuously moving organisms, and some into organisms without motion. One particle will emerge as a *Bacillus anthracis*, which produces deadly splenic fever; another will develop into a *Bacterium* the spores of which are not to be microscopically distinguished from those of the former organism; and yet these undistinguishable spores are absolutely powerless to produce the disorder which *Bacillus anthracis* never fails to produce. It is not to be imagined that particles which, on development, emerge in organisms so different from each other, possess no structural differences. But if they possess structural differences they must possess the thing differentiated, viz. structure itself." Throughout this passage it is evident that it is not anatomical but molecular structure that is referred to.

In the other passages which relate to the same subject, I venture to think that Dr. Tyndall has overlooked the distinction made by me between anatomical organization and molecular structure. When, for example, he speaks of "germ structure" in the passage quoted from his Liverpool Address, he evidently refers to molecular structure exclusively; for he gives ice as his first example, and argues that as ice possesses structure so do atmospheric germs—a proposition which I should not have thought of questioning.

The experimental evidence which we have before us goes to prove that in all the known cases in which *Bacteria* appear to originate *de novo* (that is to say, in liquids which are at the moment of their origin absolutely free from living *Bacteria*), they really originate from "particles great or small," which particles are therefore germs in the sense in which that word is used by Prof. Tyndall. To illustrate the views I myself entertain, and always have entertained, on this question, I need only refer to my paper on the origin of *Bacteria*, published in 1871. The experiments made by me at that time brought to light the then new fact, now become old by familiarity, that all exposed aqueous liquids, even when absolutely free from visible particles, and all moist surfaces, are contaminated and exhibit a power of communicating their contamination to other liquids. As regards water and aqueous liquids in general, I insisted on the "particulate" nature of the contaminating agent, and coined for the purpose the adjective I have just employed (which has been since adopted by other writers), at the same time pointing out that the particles in question were ultra-microscopical, and consequently that their existence was matter of inference as distinguished from direct observation. Dr. Tyndall has demonstrated, by the experiments to which I have already alluded, that the ordinary air also contains germinal particles of ultra-microscopical minuteness. Of the completeness and conclusiveness of those experiments I have only to express the admiration which I, in common with all others whose studies have brought them into relation with the subject, entertain. That such particles exist there can be no question; but of their size, structural attributes, or mode of development we know nothing.

Prof. Tyndall, I am sure by inadvertence, has accused me of assuming that there is some relation between the limit of microscopical visibility and what he calls the molecular limit, by which I presume to be meant the size of the largest molecule. Nothing that I have said or written could justify such a supposition. My contention is not that the particles in question are of any size which can be specified, but, on the contrary, that we are not in a position to form any conclusion as to their size, excepting that they are so small as to be beyond the reach of observation. Dr. Tyndall has taught us, first, that the optical effects observed when a beam of light passes through a particulate atmosphere are such as could only be produced by light-scattering particles of extreme minuteness; and, secondly, that by subsidence these particles disappear, and that the contaminating property of the atmosphere disappears with them. He has thus approximately determined for us the upper limit of magnitude, but leaves us uncertain as to the lower; for we have no evidence that the particles which render the atmosphere opalescent to the beam of the electric lamp may not be many times larger than those which render it germinative. Consequently the fact that the air may be rendered sterile by subsidence, while affording the most conclusive proof that germinal matter is not gaseous, leaves us without information as to the size of the particles of which it consists.

Of each germinal particle, whether inhabiting an aqueous liquid or suspended in the atmosphere, it can be asserted that under conditions which occur so frequently that they may be spoken of as general (viz. moisture, a suitable temperature, and the presence of dead proteid matter, otherwise called organic impurity) it produces an organism. If, for the sake of clearness, we call the particle *a* and the organism to which it gives rise A, then what is known about the matter amounts to no more than this, that the existence of A was preceded by the existence of *a*. With respect to A, we know, by direct observation, that it is an organic structure; but inasmuch as we know absolutely nothing as to the size and form of *a*, we cannot even state that it is transformed into A, much less can we say any thing as to the process of transformation.

Considering that it is admitted on all hands that there exist in ordinary air particles which are potentially germs, it might at first sight appear needless to inquire whether or not this fact is to be regarded as carrying with it the admission that they must necessarily possess the other attributes of organized structure. Very little consideration, however, is requisite in order to become convinced that this question stands in relation with another of fundamental importance in biology—that, namely, of the molecular structure of living material[*]. It is not necessary for my present purpose to do more than to indicate the nature of this relation. As regards every form of living matter, it may be stated that quite irrespectively of its morphological characteristics, which, as we have seen, must be learnt by the application of the various methods of visual observation at our disposal, it possesses molecular structure peculiar to itself. We are certain of this because the chemical processes of which life is made up are peculiar, that is, such as occur only in connexion with living material. Even the simplest instance that we can mention, that of the elevation of dead albumen into living (a process which in the case now before us must represent the very earliest step in the climax of development), is at the present moment beyond the reach of investigation; for as yet we are only beginning to know something about the constitution of non-living proteids. But this want of knowledge of the nature of the difference between living and non-living material in no wise impairs the conviction which exists in our minds that the difference is one of molecular structure.

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[*] The reader who is interested in this subject will find it discussed with great ingenuity by Prof. Pflüger, in his paper "Ueber die physiologische Verbrennung in den lebendigen Organismen," Pflüger's Archiv, vol. x. p. 300.

The sum of the preceding paragraphs may be stated in few words. Wherever those chemical processes go on which we collectively designate as life, we are in the habit of assuming the existence of anatomical structure. The two things, however, although concomitant, are not the same; for while anatomical structure cannot come into existence without the simultaneous or antecedent existence of the kind of molecular structure which is peculiar to living material, the proof is at present wanting that the vital molecular structure may not precede the anatomical. At the same time it must be carefully borne in mind that there is no evidence of the contrary. It is sufficient for my purpose to have shown that the existence of organized particles endowed with anatomical structure in the "atmospheric dust" has not been proved. I do not dispute its probability.

Before leaving this subject I may be permitted to add a word as to the bearing of this discussion on a question which, to myself, is of special interest—that of *contagium vivum*. According to the view which these words are understood to express, the morbific material by which a contagious disease is communicated from a diseased to a healthy person consists of minute organisms called "Disease-Germs." In order that any particle may be rightly termed a Disease-Germ two things must be proved concerning it, viz.:—first, that it is a living organism; secondly, that if it finds its way into the body of a healthy human being, or of an animal, it will produce the disease of which it is the germ. Now there is only one disease affecting the higher animals in respect of which any thing of this kind has been proved, and that is splenic fever of cattle. In other words, there is but one case in which the existence of a disease-germ has been established.

Comparing such a germ with the germinal particles we have been discussing, we see that there is but little analogy between them: for, first, the latter are not known to be organized; secondly, they have no power of producing disease; for it has been found by experiment that ordinary *Bacteria* may be introduced into the circulating blood of healthy animals in considerable quantities without producing any disturbance of health. So long as we ourselves are healthy, we have no reason to apprehend any danger from the morbific action of atmospheric dust, except in so far as it can be shown to have derived infectiveness from some particular source of miasma or contagium.

I now proceed to the second part of my communication, which relates to Prof. Tyndall's serious, but most courteously expressed, criticisms of my experiments on spontaneous generation*.

The fact that Dr. Tyndall blames me for incautiously vouching for is, "that in boiled and hermetically sealed flasks *Bacteria* sometimes appear in swarms." From multiplied experiments he concludes that this is not true, and infers that I who vouched for it was incautious. The paper referred to was one in which I, as a bystander, gave an account of certain experiments which Dr. Bastian performed in my presence. So far as relates to the fact above quoted, these experiments were, to my mind, absolutely conclusive; but inasmuch as I was unable to admit with Dr. Bastian that they afforded any proof of spontaneous generation, I followed them as soon as practicable by a series of experiments† (the only ones which I myself ever made on this subject), in which I tested the influence of two new conditions, viz. of prolonged exposure to the temperature of ebullition, and of exposure for short periods to temperatures above that of ebullition at ordinary pressure. The experiments accordingly consisted of two series, in the first of which a number of retorts or flasks charged with the turnip-cheese liquid (*i. e.* with neutralized infusion of turnip of the specific gravity 1017, to which a pinch of pounded cheese had been added), and sealed hermetically while boiling, were, after they had been so prepared, subjected to the temperature of ebullition for longer or shorter periods. In the second series the period of ebullition was the same in all cases, but the temperature was varied by varying the pressure at which ebullition took place.

The conclusion arrived at, as expressed in the final paragraph of the paper, was, that in the case of the turnip-cheese liquid the proneness of the liquid to produce *Bacteria* can be diminished either by increasing the temperature employed to sterilize it, or, if the ordinary temperature of ebullition be used, by prolonging its duration.

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* The expressions referred to are the following:—"I have worked with infusions of precisely the same specific gravity as those employed by Dr. Bastian. This I was specially careful to do in relation to the experiments described and vouched for, I fear incautiously, by Dr. Burdon Sanderson in vol. vii. p. 180 of 'Nature.' It will there be seen that though failure attended some of his efforts, Dr. Bastian did satisfy Dr. Sanderson that in boiled and hermetically sealed flasks *Bacteria* sometimes appear in swarms. With purely liquid infusions I have vainly sought to reproduce the evidence which convinced Dr. Sanderson . . . I am therefore compelled to conclude that Dr. Sanderson has lent the authority of his name to results whose antecedents he had not sufficiently examined" (Phil. Trans. vol. clxvi. p. 57). In the abstract of a lecture delivered at the Royal Institution, January 21st, 1876, similar words occur, as also in a letter to 'Nature' dated February 27th, 1876, in which Dr. Tyndall, after remarking that the experiments of Dr. Bastian, witnessed by me, were too scanty and too little in harmony with each other to bear an inference, suggests that I should repeat them.

† 'Nature,' vol. viii. p. 141.

I did not think it necessary since 1873 to occupy myself further with the subject for two reasons :—first, that I had accomplished my object, which was to show that, as a ground for believing in spontaneous generation, the turnip-cheese experiment was a failure; but secondly, and principally, because in the mean time the subject had been taken up by the most competent living observers, who had in every particular confirmed the accuracy of my results. I conclude this paper by referring shortly to some of these researches.

The first was made by P. Samuelson under the direction of Prof. Pflüger* in 1873. Its purpose was to ascertain whether it is true that certain liquids can be boiled for ten minutes without being sterilized, and, secondly, to determine the influence of prolonged periods of exposure. The flasks employed were charged with the neutral turnip-cheese liquid, and sealed while boiling in the way already described. Some were subjected to the temperature of ebullition for ten minutes, the rest for an hour, the result being that whereas those heated for the longer periods remained without exception barren, an exposure of only ten minutes was followed, in the majority of cases, by an abundant development of *Bacteria*†. At about the same period a similar series of experiments was made under the direction of Prof. Hoppe-Seyler at Strasburg. The results were essentially the same‡.

During the next year the second question which I had attempted to solve, viz. the influence of temperatures above 100° C., was taken up with much greater completeness by Prof. Gscheidlen, of Breslau§. After a *résumé* of the proofs already given by his predecessors, that certain fluids are not sterilized by boiling, and, secondly, that as means of sterilizing uch liquids the action of prolonged exposure and that of increased emperature may be regarded as complementary to each other, he proceeds o relate his own researches, the purpose of which was rather to fill up efects in the evidence than to establish new conclusions.

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* "Ueber Abiogenesis, von Paul Samuelson aus Königsberg," Pflüger's Archiv, vol. viii. p. 277. The paper is designated as a report of experiments made "im Auftrag und unter der Leitung des Geh.-Rath Professor Pflüger." I refer in the text only to those experiments which were virtually repetitions of my own. The research actually extended over a wider field.

† "Als Resultat dieser Versuchsreihe, ergab sich eine massenhafte Entwickelung von Bacterien in den meisten nur 10 Minuten lang gekochten Flüssigkeitsmengen nach 3–4 Tagen" (*loc. cit.* p. 283).

‡ "Ueber die Abiogenesis Huizinga's, von Felix Putzeys, aus Lüttich (aus dem chemisch-physiologischen Laboratorium des Herrn Prof. Hoppe-Seyler)," Pflüger's Archiv, vol. ix. p. 391. In a note appended by Prof. Hoppe-Seyler to this paper he states that he has recommended its publication notwithstanding that the results obtained were mere confirmations of those of former observers, adding "für den wissenschaftlichen Fortschritt hat nicht die Priorität des einen oder des anderen Beobachters, wohl aber die Zahl, Mannigfaltigkeit, und Zuverlässigkeit der Beobachtungen eine hohe Wichtigkeit."

§ "Ueber die Abiogenesis Huizinga's, von Richard Gscheidlen," Pflüger's Archiv, vol. ix. p. 163.

The flasks employed were capable of containing 100 cub. centims. 3½ oz.); they were charged in the usual way with the turnip-cheese quid, and exposed for short periods in chloride of calcium baths, f which the temperatures were carefully adjusted so as to obtain the equisite temperatures. It was thereby definitely proved that whereas he germinal matter of *Bacteria* can stand a temperature of 100° for five r ten minutes it is destroyed by temperatures varying from 105° to 10°[*].

In an appendix to my first paper, published in 'Nature' in the autumn f 1873, I showed that the solutions of diffusible proteids and carbo- ydrates employed by Prof. Huizinga, of Groningen, in the first of 1e valuable series of experiments[†] published by him, relating to the ubject of spontaneous generation, require a temperature above that of bullition under ordinary pressure to sterilize them. This observation as since been established by Prof. Huizinga himself on the basis of very arefully made experiments[‡], by which he has proved at the same time that the liquids in question are rendered completely incapable of producing *Bacteria*, without extrinsic contamination, by exposing them to higher temperature. The only points of difference between us, either as regards method or result, are, first, that the sterilization limit ("Grenze zur Bacterienerzeugung") fixed by me was too low, the true limit being 110° C.; and, secondly, that the experiments from which I had inferred that the liquids in question had been sterilized at lower temperatures than this were, in Prof. Huizinga's opinion, rendered inconclusive by the fact that my flasks were sealed hermetically, whereas in his exchange of air was allowed to take place, during the period of incubation, through a septum of porous porcelain. To this last objection I might perhaps have thought it my duty to answer, had it not been shown by the subsequent researches of Gscheidlen to have no bearing on the question at issue. As regards the limit of sterilization, I can entertain no doubt as to the accuracy of Huizinga's measurements, and am quite willing to accept 108° C. as the lowest temperature which could be safely employed under the conditions laid down by him.

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[*] "Es folgt aus den eben angegebenen Versuchen, nach meiner Meinung, dass in uizinga's Gemengen die Bacterien einer Temperatur von 100° 5–10 Minuten lang zu iderstehen vermögen, nicht aber einer von 105°–110° in eingeschmolzenem Glas- hre während der nämlichen Zeit" (*loc. cit.* p. 167). Here the author clearly fails to ake the necessary distinction between *Bacteria* (which, as is well known, lose their tality at a much lower temperature) and the material out of which they spring. he mixtures referred to were either the cheese and turnip liquid or solutions con- ining peptones and grape-sugar, to be immediately referred to. As affording an elegant monstration that in the turnip-cheese liquid it is the cheese and not any other con- ituent which contains the resistant element, the following form of experiment is orthy of notice:—A tube, A, drawn out and closed at both ends, is fused into the open outh of a second tube, B, of which the opposite end is drawn out and closed in a milar manner. In this way a compound tube is formed which is divided by a coni- l septum into two chambers, A and B. A small knob of glass having been previously troduced into the chamber B, the septum can be easily broken by shaking the tube. ith tubes so prepared two experiments are made. In Experiment 1, compartment is charged with infusion of cheese, sealed, and then exposed to a temperature of 0° before it is united to the compartment B. In like manner B is charged with utral decoction of turnip, so that when the compound tube is complete it contains eese in one compartment, turnip in the other. If, after boiling for ten minutes, it placed in the warm chamber its contents remain barren. In Experiment 2 the periment is varied by simply omitting the preliminary heating of A. The compound be is boiled as before, but now its contents promptly give evidence that the condi- ns are present for an abundant development of *Bacteria*.

[†] Prof. Huizinga's papers on the question of Abiogenesis are four in number. The ferences are as follows:—Pflüger's Archiv, vol. vii. p. 225; vol. viii. pp. 180, 551; l. x. p. 62.

[‡] The solution employed in these experiments was neutral and contained, in addi- n to the requisite inorganic salts, 2 per cent. of grape-sugar, 0·3 per cent. of soluble

It will be understood that in bringing these facts before the Society my only purpose is to show, as I trust I have done conclusively, that the statements which Dr. Tyndall in 1876 characterized as incautious, and which he virtually invited me to retract, had been two years before confirmed in every particular by experimenters of acknowledged competence.

November 30, 1877.

ANNIVERSARY MEETING.

Sir JOSEPH HOOKER, C.B., K.C.S.I., President, in the Chair.

Mr. Abel, for the Auditors of the Treasurer's Accounts on the part of the Society, reported that the total receipts during the past year, including a balance of £150 18*s*. 11*d*. carried from the preceding year, amount to £7056 14*s*. 4*d*.; and that the total expenditure in the same period amounts to £6123 3*s*. 3*d*., leaving a balance at the Bankers' of £923 4*s*., and £10 7*s*. 1*d*. in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

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starch, 0·3 per cent. of peptones, and 1 per cent. of ammonic tartrate. As in my experiments, the flasks were heated in a Papin's pot, of which the temperature was 102° C. Even after half an hour's exposure to this temperature all the flasks became in two or three days "stark trübe und voll Bacterien" (third paper, p. 555, January, 1874).

The Secretary read the following Lists :—

Fellows deceased since the last Anniversary.

On the Home List.

Thomas Snow Beck, M.D.
James Scott Bowerbank, LL.D.
John Butter, M.D.
Ardaseer Cursetjee.
Major-General Frederick Marow Eardley-Wilmot, R.A.
Sir William Fergusson, Bart.
David Forbes, Sec.G.S.
Robert Were Fox.
John Peter Gassiot, D.C.L.
Right Hon. Robert C. Nisbet Hamilton.
John Heywood Hawkins, M.A.
William Henry Hyett.
Major-Gen. Sir Henry James, R.E.
Julius Jeffreys.
Robert Lee, M.D.
Henry Minchin Noad, Ph.D.
John Russell Reeves, F.L.S.
Alfred Smee, F.R.C.S.
Robert Swinhoe, F.Z.S.
William Henry Fox Talbot, LL.D.
Charles Townley, F.S.A. (Nov. 4, 1876).
Samuel Warren, D.C.L.
Charles Woodward.

On the Foreign List.

Karl Ernst von Baer.
Georg Adolph Erman.
Urbain Jean Joseph Le Verrier.

Fellows elected since the last Anniversary.

John Duke, Lord Coleridge, M.A.
Prof. James Dewar, M.A.
Sir Joseph Fayrer, M.D., K.C.S.I.
Rev. Norman Macleod Ferrers, M.A.
Thomas Richard Fraser, M.D.
Right Hon. Sir Henry Bartle Edward Frere, K.C.B.
Brian Haughton Hodgson, F.L.S.
John W. Judd, F.G.S.
William Carmichael M'Intosh, M.D.
Robert M'Lachlan, F.L.S.
Prof. John William Mallet, Ph.D.
Henry B. Medlicott, M.A.
Henry Nottidge Moseley, M.A.
Prof. Osborne Reynolds, M.A.
William Roberts, M.D.
Prof. James Thomson, LL.D.
Prof. William Turner, M.B.

The President then addressed the Society as follows :—

GENTLEMEN,

FOLLOWING precedent, I have at the commencement of my Annual Address to record the losses by death of eminent Fellows of this Society which have taken place since the last Anniversary. Though, happily, these losses are not so numerous as they have been in late years, the number amounts to twenty-three, and the list includes the names of men of great distinction in science, and among them of one to whom the Society is under lasting obligations for his active interest in its welfare during upwards of a quarter of a century. Need I name Mr. Gassiot, the founder of the Scientific Relief Fund, the munificent subsidizer of the Kew Observatory, and the ever-ready and liberal promoter of scientific investigation—Mr. Fox Talbot, the discoverer of photogenic drawing (the Talbotype process), proved to be the fruitful parent of photography—Sir Henry James, under whose administration the operations of the Ordnance Survey of Great Britain were greatly extended, and its resources utilized in various ways, especially through the application of scientific processes all tending to the advancement or diffusion of knowledge—Mr. Robert Were Fox, eminent for his researches on the temperature and the magnetic and electrical condition of the interior of the earth, especially in connexion with the formation of metallic veins, and who was, further, the inventor of some and improver of other instruments now everywhere employed in ascertaining the properties of terrestrial magnetism? In Sir William Fergusson we have lost a surgeon of rare ability and manual dexterity and an operator of great repute; in Mr. David Forbes an accomplished traveller, chemist, and mineralogist; and in Dr. Bowerbank a naturalist of the old school, whose enthusiasm and genial encouragement kindled into a flame the scientific spark that lurked in the breast of many an amateur. There have further been removed by death from the list of Foreign Fellows two recipients of the Copley Medal, the venerable Von Baer and the comparatively young Le Verrier, together with a traveller and physicist of rare attainments, Erman, the narrative of whose travels is one of the richest storehouses of scientific information that has hitherto been given to the world in the narrative form.

Finance.—As heretofore, I must refer to our Treasurer's Report for evidence of the satisfactory condition of the Society's finances. Not but that this is a matter that requires constant vigilance, as the demands upon our pecuniary resources annually enlarge, owing mainly to the rapid increment of matter brought before us and found worthy of publication in our Transactions and Proceedings, and, above all, to the number of expensive illustrations which accompany many of them. This, and the prospect of the results of the Government Fund for the encouragement of research being laid before the Society for publication, appeared to me to render it desirable that a Committee should be appointed to inquire into and report upon the receipts and expenditure of the Society, and that the subject of the outlay on printing and paper should be referred to the Library Committee for report, together with that of the compilation of the Catalogue of Scientific Papers, the labour and expense of which were likely to increase with that enormous development of scientific literature which characterizes this century.

On the recommendation of that Committee, our printing has been transferred to a well-qualified firm of printers, on such conditions as will enable us, we hope, to effect an important saving in our annual charge for printing. It is thought, moreover, that the compilation of the Catalogue of Scientific Papers, which, though no part of our ordinary work, had been voluntarily undertaken and paid for by the Society, and diligently conducted under the supervision of your officers, should not be allowed to press unduly upon our resources, and that the time had come when application should be made to the Government Fund for aid to enable us to meet the increasing demands on our income for the work of the Catalogue.

And further, as bearing on the question of finance, your Council have resolved that the difference between the amount of Life composition payable by newly-elected Fellows who have and those who have not previously to election contributed a paper to the Transactions should cease, and that a part of the funded property of the Society should be invested in secure Stocks yielding a larger interest than the Government funds.

Presents.—It is always with peculiar pleasure that I announce the presentation of good portraits of scientific worthies. Two in oils, received during the vacation, are now hung on our walls: one of Sir John Herschel, a very faithful likeness, presented by our Fellow, Mr. John Evans; the other, presented by our late Secretary, Dr. Sharpey, is that of Haller, the physiologist, anatomist, botanist, and poet, whose genius and labours were the admiration of his contemporaries, as they have been ever since of his successors. It is not without pride that our countrymen can record the facts that an English sovereign, George II., was the first who recognized the merit of Haller, the Swiss, by bestowing on him his earliest preferment, a professorship in Göttingen, and ever after showing him every mark of respect, and that, on a subsequent occasion, an English University, Oxford, offered him a professorship. The portrait is an excellent one, in perfect preservation, and forms a most valuable addition to our gallery.

I have also to inform you that a sum of £500 has been contributed anonymously by five Fellows to the Society's funds for general purposes, and that our Foreign Secretary has proposed that his office should, as long as he holds it, be regarded as an honorary one, with charge of the Society's foreign correspondence. This very liberal proposal was accepted by the Council, only in so far as resolving that the Foreign Secretaryship should be placed on the same footing in respect of salary as it was before the year 1865; that is to say, that the honorarium should be limited, as in former years, to the proceeds of the original endowment.

Our Fellow, Dr. William Farr, has presented to the Society an annotated copy of Thomson's 'History of the Royal Society,' containing the dates of death of Fellows who died subsequently to the publication of that work, as far as these could be obtained from the Society's Minutes and from printed books, together with a complete chronological list of all the Fellows admitted since 1812 down to 1876. These, with other documents which he has added, enable Fellows to ascertain the names, and dates of birth and death, of every person admitted into the Society since its origin, and hence, to a great extent, supplies valuable data for determining the vitality of scientific men at different periods. In his letter accompanying these very valuable documents Dr. Farr observes that the records of the Royal Society were allowed for years to remain defective as to particulars which were formerly accurately recorded, and that Halley and others seemed to have been alive to the importance of such facts relating to the scientific men of their age. In future, the date and place of birth of Fellows will be registered regularly and accurately, in accordance with Dr. Farr's excellent suggestion, for which, as for the documents, a unanimous vote of thanks was returned by your President and Council.

The Catalogue of Scientific Papers.—In my last year's Address I informed you that the Lords of the Treasury had granted the funds necessary for printing the decade 1864–73 of the Catalogue of Scientific Papers; and I have now to announce that the first (the seventh of the series) of the two volumes of which it will consist is published. It contains more than a thousand pages. The expediency of the Society's further undertaking the compilation of an "Index of Subjects" having been urged upon the Library Committee, was carefully considered by them. To this end the members were supplied with printed specimens of a well-considered plan adapted to the decade 1864–73, with the request that they would favour the Council with their opinion upon it; when it appeared that, owing to the number of subjects often comprised in one paper, and the differences of opinion as to which of these were worthy of citation, and under what name, the task would be one of prodigious labour and unsatisfactory result, and far beyond the Society's means. The printing of the eighth volume is steadily progressing, together with the compilation of the decade for 1874–83.

The Meteorological Council.—The Report of the Treasury Committee of Inquiry into the working of the late Meteorological Office was published last summer. It includes that of the Committee of this Society (none of the members of which had served on the Treasury Committee); and the recommendations of the two bodies are almost identical. As a member of the former, and cognizant of the views of the Government as to the future of the Office, I may state that those views were from the first, and throughout, favourable to giving a more scientific character to the work than it had hitherto possessed, recognizing the principle, that its aim and endeavour should be to advance meteorology as a science, while directing and controlling all such practical operations as were required for the public service. The main difference between the recommendations of the Treasury Committee and our own is that we favoured the retention of the Office under a department of the Crown, with a Government officer as Director, in preference to leaving it subject to a Committee or Council of Control. The Treasury Committee, influenced by the evidence of very eminent scientific men to this effect, that meteorology was not as yet in a scientific condition, and that to render it so required the combined labours of men with various attainments, as also by the fact that there was no department of Government capable of controlling purely scientific investigations, recommended that, as a tentative measure, a modified Committee of Control (to be called a Council) should replace the old Committee, and that the Royal Society should be asked to nominate the members, and, after a period of five years, to review their labours.

Other recommendations, in which both Committees concurred, were, that ocean meteorology should be transferred to the Admiralty, that the maxims which determine the issue of storm-warnings should be put in a clear shape and issued to the public, that the number and position of both the continuously-recording and the eye-observing stations should be revised, that the latter should be increased so as to satisfy the claims of the Registrars-General, Medical Council, Agricultural Societies, and other bodies, and that a fair approximation to the meteorological condition of the whole British Isles should be daily obtained and published.

Far more important to us, however, than these practical measures, is the strong expression of opinion on the part of both Committees that scientific hypothesis and discussion should be pursued as a duty incumbent on the Office, and that, to this end, an annual grant should be made for the purpose of remunerating investigators, selected by the Council, on a scale proportionate to the work performed.

At the request of H.M. Treasury, and in communication with them, your Council drew up the following suggestions for the administration of the Meteorological Office, which, having been approved, are now put in practice:—That the Office be in future administered by a paid Council, consisting of a Chairman and four effective members, together with, as an *ex officio* member, the Hydrographer of the Navy, whose services were rendered necessary by the Admiralty having declined to undertake the ocean meteorology; that £1000 should be granted for the remuneration of the Members of Council, who should be persons in a position to devote adequate time and attention to the duties of the Office, and to the inauguration of investigations and experiments designed to place meteorology on a scientific basis, to advance it, and to promote its usefulness to the community; that paid inspectors of stations should be appointed for Scotland and Ireland, and £500 be granted for this purpose; that a sum of £1000 per annum be granted for the payment of individuals, to be selected by the Council, to be engaged in special scientific researches; and that £1500 be granted for new land-stations, and £500 for the extension of telegraphy to Sundays. The result of these new measures will be to raise the annual grant for the Meteorological Office from £10,000 to £14,500.

Your Council having further been requested to nominate the effective members of the Meteorological Council for the approval of the Lords of the Treasury, proceeded to do so in accordance with the spirit of the resolutions which gave scientific research so prominent a position—keeping in view, at the same time, the necessity of obtaining the services of as many members of the old Committee as possible, their knowledge of the details of the Office being at first indispensable, and their efficiency already proved. The result has been the appointment of Mr. Henry Smith, Savilian Professor of Geometry at Oxford, as Chairman, and your Senior Secretary, Mr. F. Galton, Mr. De La Rue, and General Strachey as the other members. The services of Mr. Scott, who has so long and so ably directed the practical operations of the Office, and of Mr. Toynbee, whose labours in ocean meteorology are so well known to you, and of the other officers being all retained, nothing would seem to be wanting, in men or money, to develop the science of meteorology, and to supply the public with data for all the useful purposes contemplated in the establishment of the Meteorological Office. It is to be hoped that the tentative measure thus inaugurated will lead, in five years, to the constitution of a national Meteorological Office under the undivided control of a man of high scientific attainments.

Government Fund of £4000 per Annum for Five Years.—The constitution of the Committee for the administration of this fund, under the authority of the Lord President of the Council, has been provisionally settled, and as much of the first year's grant as was available for the last quarter of the financial year March 31, 1876, to April 1, 1877, was allotted in March last.

The first meeting of the Committee took place on January 11th, when it was resolved:—that four subcommittees should be constituted—namely, (A) Mathematics, Physics, and Astronomy, (B) Biology, (C) Chemistry, (D) General Purposes; and that all applications for grants should be addressed to the Secretaries of the Society, and referred by them to one of the first three subcommittees for examination and report and recommendation; that the subcommittees' reports and recommendations should be printed and circulated among the members of the General Committee not less than one week before the meeting at which the grants would be discussed; and that the grants applied for should be limited to sums required for a period not exceeding twelve months. It was further resolved that a report of progress should be required of the recipient at the end of the year in which the grant was made, and that instruments of permanent value purchased out of the

Fund, or supplied by the Government on the recommendation of the Royal Society, should be regarded as the property of the Government.

The Committee meetings are fixed for February in each year; those of the subcommittees will take place whenever summoned by the Secretaries of the Society; and notice has been given that applications for grants are to be sent to the Secretaries of the Royal Society not later than December 31st in any year.

At the meeting of the General Committee in March last the subcommittees reported that 102 applications had been received, and that the amount applied for was £14,459. Of the 102 applications 33 were approved, and sums of £300 and under (the total being £2220 1*s.* 6*d.* for instruments, assistance, and materials, and £1810 for personal remuneration) were granted.

The results of this step towards the endowment of research will, I hope, be narrowly watched, in the interests both of science and of this Society, which, in undertaking to administer for the Government a sum so largely devoted to personal remuneration, has assumed a very onerous responsibility, and largely increased the burthen of your Secretaries.

Reports of Naturalists sent by the Society to Rodriguez and Kerguelen Island.—These are being printed uniformly with our Transactions, under the editorship of Dr. Günther and your President. They will consist of a series of papers, illustrated with plates, on all branches of the natural history of the islands, contributed by the naturalists themselves and various coadjutors, whose services are gratuitous. The cost of printing will be defrayed by the liberality of your Treasurer, and some of the plates have been presented by the contributors.

The Polar Expedition.—The scientific results of the Polar Expedition, and especially the biological, appear to me to have, in most departments, quite come up to our expectations; and considering that but one season was available for collecting and observing (and we all know how short that is in the arctic regions), they are indeed most creditable to the gentlemen who contributed them. Geology has proved by far the most prolific field of research. Perhaps Botany comes next, and this, and the insects which have been worked up by Mr. M'Lachlan, prove that, between 80° and 83° N., in Grinnell Land, the conditions for the existence of these organisms are far more favourable than are those of lands a long way to the southward.

The floras of the series of channels between 80° and 83° N., the shores of which have been botanized by the officers of the Polar Expedition, have yielded upwards of 70 flowering plants and ferns, which is a much greater number than has been obtained from a similar area among the polar islands to the south-westward, and is unexpectedly large. All are from a much higher latitude than has elsewhere been explored botanically, except the islets off the extreme north of Spitzbergen. The species are, with two exceptions, all Greenlandic. The exceptions are *Androsace septentrionalis*, which, though found in the northern regions of all the continents, has never elsewhere been seen north of lat. 72°, and *Pedicularis capitata*, an American and North-Asiatic species, not hitherto recorded north of the same parallel.

Spitzbergen, stretching from latitude 76° 30′ to 80° N., quite to the south of the positions here referred to, has contributed not more than 100 flowering plants and ferns, notwithstanding that its west coast is washed by the Gulf-stream, and that its shores have been diligently explored by many trained collectors. Fifteen of the plants collected by the Expedition have not been found anywhere in Spitzbergen. Compared with Melville Island, in lat. 75° N., and Port Kennedy, in 72° N., the contrast is even more striking, these well-hunted spots, both so much further south, yielding only 67 and 52 species respectively.

This extension of the Greenland flora to so very high a latitude can only be accounted for by the influence of warm currents of air, or of the air being warmed by oceanic currents, during some period of the summer; and I look with great interest to the meteorological observations made during the voyage, which are being discussed by Sir George Nares, who hopes to have them completed in a couple of months. The observations on the temperature of sea-water will, he expects, give new information; and the study of certain warm gales and warm currents that were observed in lat. 82° and 83° N. can hardly fail to increase our knowledge of the local climate.

May not these phenomena of vegetation and temperature indicate the existence of large tracts of land clothed with vegetation in the interior of Greenland, far within the mountain-ranges of its ice-clad coast, and protected by these from the heavier snowfalls and from the accumulation of glacial ice which borders that island on all sides?

The fossil plants collected have been examined and reported upon by Professor Heer. Of these the most important are the Miocene. They consist of 25 identifiable species, of which 18 are known Arctic Miocene fossils. All but one had been previously found in Spitzbergen. The most interesting of them is the Conifer, assumed to be identical with the existing American "Bald Cypress," *Taxodium distichum*, a plant which is now confined to Eastern North America, from lat. 39° southwards, and to which specimens found in the Miocenes of France, Italy, Prussia, Greenland, and N.W. America have also been referred.

Professor Heer further thinks that he has identified the remains of a Spruce with the European and Asiatic Norway Spruce (*Abies excelsa*), which occurs as a fossil only in Postpliocene beds. Its existence in the Miocene period only in such a high latitude would indicate that it is a polar form which has migrated southward in more recent times.

This tracking of the Miocene flora so far to the northward was one of the principal scientific objects to be accomplished by the Polar Expedition; and the fact that the character thereof continues to be neither polar nor arctic, but temperate, supports the hypothesis that during the era in question a vegetation analogous to that now prevailing in the temperate latitudes entirely covered the north-polar area of the globe.

Other branches of Geology have yielded very valuable results in the hands of Mr. Etheridge, who has worked up the very large number of Palæozoic fossils collected especially by Capt. Feilden. These, with the Carboniferous, Miocene, and Postpliocene fossils, animal and vegetable, and the abundant rock specimens, have thrown more light on the former condition of the circumpolar regions than perhaps all the collections of previous expeditions.

Capt. Evans has been so good as to supply me with the results of the magnetical observations made during the voyage, which were in general accordance with those of the American expeditions to Smith's Sound. Nearly continuous hourly observations of the Differential Declination Magnetometer were taken throughout the winter from October to April. With an inclination of nearly 85°, and a horizontal force of 1·13, the westerly declination disturbance occurred usually between 9 A.M. and 5 P.M., the easterly between 9 P.M. and 5 A.M., the arc ranging through 8°. The greatest range (Feb. 19, 1876) reached 5° 48′; the lowest (July 12, 1876) was scarcely 7′. Compared with the results of previous expeditions, we find that, at Rensselaer Harbour, with horizontal force of 1·14 and inclination nearly 85°, the ranges were respectively 4° 52′ and 1° 1′; while at Port Bowen, with horizontal force 0° 46′ and inclination 88°, they were 11° 56′ and 0° 35′ respectively. The mean daily range of declination was 86°·8, and mean declination 101° 42′ W.

The observers were on the alert to observe any signs of connexion between the auroral displays and declination-disturbances, but to no purpose; for, as with Parry in Port Bowen, and Kane in Rensselaer Harbour, no evidence was discoverable. On the other hand, various previous voyagers have registered a marked connexion, as at Port Kennedy by Maguire, at Point Barrow by M'Clintock, and in the Spitzbergen seas by Weyprecht. Considering that there can be no doubt as to the trustworthiness of all these observers, a decisive solution of the question is greatly to be desired.

Sir William Thomson and Prof. Everett have examined the few observations made for the amount of atmospheric electricity, with the result of finding that they confirm the observations of former explorers.

Sir George Nares has obligingly sent me a *résumé* of some of the principal meteorological results, and their comparison with those taken at Polaris Bay in 1871–72: for example:—

	Mean annual pressure. in.	Mean annual temperature. °	Minimum temperature. °
'Alert,' Floeberg Beach....	29·869	−3·467	−73·75
'Discovery,' Discovery Bay.	29·887	−3·932	−70·8
Polaris Bay..............	29·970	+4·196	−45·5

Minimum temperature of earth 20 inches beneath surface, −13°·0.

The warmer temperature at Floeberg Beach was due to its exposure to the warm winter gales, from which Discovery Bay was cut off. The still warmer temperature of Polaris Bay is partly attributable to there being some uncovered water in the neighbourhood.

The barometer indicated and foretold changes in the weather as in temperate regions.

Making due allowance for unavoidable sources of error, the temperatures of the sea observed on the west shores of Smith's Sound prove the existence of a stratum of cold outer water (temperature about 29°) lying between the locally heated surface-water and a depth of twenty to thirty fathoms, flowing southward in summer, as also of an underlying stratum with a temperature of about 30°. This latter was not found near Floeberg Beach; but, coupled with the 1872 observations of the 'Polaris,' which showed a temperature of 32°·8 at 203 fathoms in lat. 80° 44′ N. (midway between Franklin and Hals Islands, in Robison Channel), and 32°·1 at 17 fathoms in Polaris Bay, it would appear that the warm underlying water forces itself to the northward on the east side of Robison Channel. Its entrance into the polar sea or not will depend on the depth of water at the north end of the channel. They also prove the non-existence of a lower temperature of the water than 28°·8 at a depth below 275 fathoms in Smith's Sound or Baffin's Bay. The coldest portion of the arctic water appears not to affect Hayes Sound or Discovery Bay to so great an extent as that of the direct channel.

The Rev. Dr. Haughton, to whom the tidal observations of the 'Discovery' and 'Alert' were entrusted, informs me that he has completed the preliminary discussion of the whole, and hopes to present the final results of those of the 'Discovery' to this Society before June next. He remarks that the 'Discovery' was better provided for observations than the 'Alert,' and, fortunately, her position was better also, as she lay nearer the head of the tide at Cape Payer. The officer charged with the observations, Lieut. Archer, made them hourly for seven months, with only six days of interpolation. The officers of the 'Alert' were able to make hourly observations for two months only, with fifteen days of interpolation.

Dr. Haughton has already arrived at the following general conclusions:—1. The tide which comes down Smith's Sound from the north is generically distinct from the Behring's Straits tide and from the Baffin's Bay tide. 2. It must therefore be the East-Greenland Atlantic tide;

and consequently Greenland is an island. 3. This new tide contains a sensible tertio-diurnal component of much interest.

The mean coefficients of the components are:—

Semidiurnal tide .... =50·6 inches.
Diurnal tide .... = 6·9 „
Tertio-diurnal tide .... = 4·5 „

The 'Challenger' Expedition.—You will hear with gratification that the Lords of the Treasury, after advising with your Council, have appropriated the munificent sum of £25,000 for publishing the biological results of the voyage in a style and with a completeness worthy of the Expedition and the nation. Adopting a course as wise as it is liberal, Sir Wyville Thomson has, with the approval of your Council and the Government, chosen for his collaborators the ablest specialists, irrespective of their nationality. It is creditable to our country that, with but few exceptions, it has supplied thoroughly competent and willing workers in most of the departments; and association with such foreign naturalists as Agassiz and Haeckel cannot fail to be gratifying to themselves and assuring to the public. I had the advantage of inspecting the Echinodermata in Professor Agassiz's charge in the Peabody Museum at Harvard College, and of learning the progress he had made in the examination of the vast body of materials entrusted to him. These, he informed me, far surpassed Sir Wyville's estimate in number of species and of interesting and novel forms; and I was surprised to find that the whole had already been sorted, that the greater part was named and ready for return to Edinburgh, and that nearly a dozen exquisite lithographic plates of new and rare forms were prepared for publication.

Sir Wyville Thomson informs me that he is already far advanced towards the publication of two quarto volumes, and that he estimates the whole being completed in fourteen, of the form and size of the Philosophical Transactions.

Transit of Venus.—Sir George Airy has been pleased to inform me that the inferences from the telescopic observations of the transit of Venus, made in the British expeditions for records of that phenomenon, under the superintendence of the Astronomer Royal, have now been published—first, in response to an order of the House of Commons; secondly, in a more elaborate communication to the Royal Astronomical Society. The number of districts of observation was five, but each of these included a principal and some subordinate stations. The number of observers was eighteen, and as some of them observed both ingress and egress of Venus at the Sun's limb, the total number of observations was fifty-four. The concluded value of equatorial mean solar parallax was 8″·754. The calculation of the photographic records of the transit is advancing rapidly.

The Report on the results of the total Solar Eclipse of 1875, announced in my last year's Address as being drawn up by Mr. Lockyer, is now in our hands.

The Harvard College Observatory (U.S.).—During my recent visit to the United States, I was for a short time a guest at the Cambridge Botanic Garden, and consequently in close proximity to the fine Observatory of Harvard College, to which I paid several visits, being most kindly received by Professor Pickering, successor to the distinguished astronomer, W. C. Bond. The system carried out in this observatory is known to British Astronomers to be so productive of good results that I felt sure that some account of it would be acceptable to the Fellows of the Royal Society; and I therefore availed myself of Mr. Pickering's good offices to obtain a few particulars.

The current work of the Observatory is threefold, and consists of observations with the 15-inch equatorial, with the 8-inch aperture meridian circle, and communication of true time-signals to the public.

The principal work of the equatorial is photometrical, an instrument having been devised by the Astronomer by which two stars may be compared directly without using an artificial star as an intermediate step in the measurement. By this means the relative brightness of the components of numerous double stars, including some having only very faint components, as also the relative brightness of the satellites of Jupiter and Saturn, has been determined.

At the time of my visit Prof. Pickering was engaged in a special study of the newly discovered satellites of Mars, one of which, the outer, I had the satisfaction of observing through the equatorial. Their brightness he had determined by three very ingenious methods:—1st, by comparison with an image of Mars shining through a very minute circular hole placed at the focus of the telescope; 2nd, by comparing the satellite with a minute image of Mars formed in the field of the large telescope by a small auxiliary telescope; 3rd, by reducing the light of the inner satellite by one, two, or three plates of microscope-glass, until its brightness was equal to that of the outer satellite. Of these methods the second showed that the outer satellite does not partake of the red colour of Mars.

The meridian circle was, or had lately been, in use for the following purposes:—1st, the determination of the position of all stars brighter than the 9th magnitude contained in the zone 50°–55° N.; 2nd, observations of Mars during the opposition of last summer for the solar parallax; 3rd, observations of a list of composite stars, at the request of Mr. Gill, for determining the solar parallax by means of Ariadne; 4th, preparations are being made for the determination of the absolute position of a catalogue of stars, independently of all previous observations, and, 5th, for the publication of a catalogue of polar stars observed in 1872–1873; 6th, with the assistance of the U.S. Coast Survey, a beginning has just been made of the measurement of all stars in the northern hemisphere brighter than the 6th magnitude, whose positions have not recently been determined with precision.

Time-signals for the meridian of Boston are sent by telegraph every two seconds from the Observatory; they are used by the local railways, are transmitted over a large area of New England, and they strike the noon-bells in Boston and in many of the smaller towns.

Besides the above, several thousand observations for atmospheric refraction were made, with the assistance of the Rumford Committee, during last summer with a micrometer level, simple in construction and accurate and rapid in action, invented by Mr. Pickering.

United States' Scientific Surveys.—Of the many surveys of the United States territories undertaken, some by the Central Government, others by State governments, and still others by private enterprise, more or less aided by public funds, none has effected so much for science as that directed by Dr. Hayden. Its publications, distributed with great liberality, are in every scientific library, and its Director is honoured no less for the energy and zeal with which he has laboured as a topographer and geologist, than for the enlightened spirit in which he has sought to render the resources of the Survey available for the advancement of all branches of natural knowledge by every means in his power, and with admirable impartiality.

Having obtained an extended leave of absence from my official duties at the Royal Gardens, I, at the close of our last session, accepted an invitation from Dr. Hayden to join his survey, and, in company with our Foreign Member, Prof. Asa Gray, to visit, under his conduct, the Rocky Mountains of Colorado and Utah, with the object of contributing to the records of the Survey a report on the Botany of those States.

I have thus had some opportunity of learning for myself the extent and value of the operations of the Survey, which are so interesting that I venture to think a brief sketch of its rise and progress and a few of its results may be acceptable to you.

When the territory of Nebraska was admitted into the Union in 1867, Congress set apart an unexpended balance of £1000 for a Geological Survey of the new State; and Dr. Hayden, then a young man who had distinguished himself as an indefatigable palæontological observer and collector (in various expeditions since 1853), was appointed to conduct it. In 1868 the operations of the Survey were continued, and carried westward into the Rocky Mountains of Wyoming, the rich Tertiary and Cretaceous beds of which were examined and described in detail, and the famous Yellowstone district, with which Dr. Hayden's name will ever be associated, was reconnoitred. The value of the Survey was immediately appreciated, and in 1869 a large appropriation was voted by Congress for placing it on its present footing under the supervision of the Secretary of State for the Interior. In 1869 and 1870 operations were carried on in Colorado and New Mexico; and full reports on the meteorology, agriculture, zoology, and palæontology of these regions, of great interest and importance, were drawn up and subsequently published. In 1871 the detailed survey of this Yellowstone district was begun, and those marvellous natural features were carefully studied, which have excited the liveliest interest in Europe, and have induced Congress, on Dr. Hayden's representations, to appropriate the whole area as a Government reserve, thus securing to naturalists free access to natural phenomena which in other places, both in Europe and America, are too often monopolized by speculators and closed to the public.

In 1872 the Survey was further extended, and was organized into two corps, each provided with a topographer, geologist, mineralogist, meteorologist, and naturalist, and the States of Idaho and Montana were embraced in its operations; in 1873 it was pushed into Colorado, thence into Utah, and on its completion in 1876, an area of not less than 70,000 square miles, much of it exceedingly mountainous, had been included in the Survey.

The literature of the Survey consisted, in 1876, of 41 volumes, classified as follows:—1, annual reports, with maps and sections; 2, bulletins for giving speedy publicity to new facts; 3, miscellaneous publications, comprising tables of elevations, catalogues of plants and animals, and meteorological data; 4, monographs on various branches of natural history, especially palæontology, copiously illustrated with admirable plates in quarto, among which are the works of Leidy, Lesquereux, Coues, C. Thomas, Cope, Parry, Meek, Packard, Silliman, Hayden himself, and others, all of whom are well known on this side of the Atlantic; lastly, the number of photographs now exceeds 4000, and includes, besides geological and geographical features of great interest, views of ancient architectural remains, and of 1200 Indians, belonging to 74 tribes.

In giving these particulars I speak from some personal knowledge. I wish that the same could be said of the local habitation of the Survey and its museum, which, I am assured, contains a very extensive and instructive collection; but these are at Washington, and my pressing duties here and at Kew prevented my visiting the federal capital.

The most important scientific results hitherto derived from the labours of Dr. Hayden and his parties are unquestionably the geological: such as the delineation of the boundaries of the Cretaceous and Tertiary seas and lakes that occupied more than one basin of the mountains of Central N. America, and the marvellous accumulation of fossil Vertebrates that these ancient shores have yielded. Over an area of many hundred thousand square miles in North America there have been found, within the last very few years, beds of great extent and thickness, of all ages from the Trias onwards, containing the well-preserved remains of so great a multitude of flying, creeping, and walking things, referable to so many orders of plants and animals, and often of such gigantic proportions, that the palæontologists of the States, with museums vastly larger than our own, are at a loss for space to exhibit them. So common indeed are some species, and so beautifully preserved, that I saw numbers of them, especially insects, plants, and fishes, exposed for sale, and eagerly purchased by travellers, with confectionery and fruit, at the stalls of the railway stations, from the eastern base of the Rocky Mountains all the way to California.

An examination of some of these fossils has brought to light the important fact that in North America there is no recognized break between the Cretaceous and Tertiary beds. This is due to the interpolation of a vast lignitic series the fossils of which furnish conflicting evidence. Concerning this series Dr. Hayden, who has traced it over many hundred miles, observes* that the character of its palæontological, as well as of its strictly geological, results is such, that whether the entire group be placed in the Lower Tertiary or Upper Cretaceous is unimportant, and that the testimony of palæontologists will probably always be as conflicting as at present.

Professor Marsh, of Yale College, Newhaven, one of the highest authorities in America, has found that not even invertebrate fossils afford a satisfactory solution of the difficulty. "These," he says, "throw little light on the question;" and he is obliged to assume that "the line, if line there be, must be drawn where the Dinosaurs and other Mesozoic Vertebrates disappear, and are replaced by the Mammals, henceforth the dominant type."

This last passage I have taken from the lucid address of Professor Marsh to the meeting of the American Association for the Advancement of Science, held last autumn at Nashville, to which I must refer for an exposition of the riches of the fossil Vertebrate fauna of these regions, of the convincing proofs they afford of the doctrine of Evolution, and of the light they throw on the introduction, succession, and dispersion of existing organisms in the New World. Among the suggestive observations with which this address abounds is another in reference to this question of the disputed horizons of the Cretaceous and Eocene beds—namely, its dependence on the relative value to be given to evidence derived from plant and animal remains. He concludes that plants afford unsatisfactory measure of geological periods as compared with animals—a conclusion at which I had long ago arrived. We agree further that a chief cause of this difference of value is the less complex organization of plants, which hence furnish less evidence of the influences of environing conditions; to which might be added the feeble conflict among the higher members of the vegetable kingdom as compared with the vertebrates, their stationary habits, and the duration of similar, if not identical, forms through long geological ages, which has always appeared to me to be one of the most signal characteristics of the early condition of the higher plants as compared with the higher animals. Other, and perhaps even more cogent, reasons for plants being so little satisfactory is, that their reproductive organs, those upon which the classification is principally based, are rarely preserved, and seldom in connexion with the vegetative organs, which are abundantly preserved; and that, with regard to these, the vegetative organs, their prevalent and best-preserved characters, outline and venation, vary in individual species to a surprising degree, and, being repeated in groups otherwise in no way related, become too often fallacious guides.

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* Report of Geological Survey, 1874, p. 20.

Another result, previously obtained in respect of other organisms, but ably worked out by Professor Marsh as regards the Vertebrates, is that all the Tertiary beds of North America—Eocene, Miocene, and Pliocene—are of older date than the corresponding beds in Europe. This, though apparently supported by his conclusions that the main migrations of animals took place from the American to the Asiatic continent (which he deduces from the American, as compared with the European, life-histories of the Edentata, Marsupialia, Ungulata, Rodentia, Carnivora, and even Primates), is a very bold generalization. Without presuming to question the abundance and teachings of the American data, I cannot but think that his theory of migration is, in the present state of palæontology, premature, especially under our almost absolute ignorance of the Vertebrate fossils of the continents of Asia and Africa. The prodigal palæontological wealth of the United States, as compared with the poverty of that of Europe as yet known, may be likened to that of a metropolitan museum or library as contrasted with a provincial collection; and with regard to Central Asia especially, there are indications, in the narratives of travellers and the reports of natives, of vast accumulations of vertebrate fossils there existing. These may revolutionize our present ideas, as Falconer's and Cautley's discoveries in the outer Himalayas did those of our predecessors; and he would be a rash speculator who, having studied what is known of the physical geography of Asia north of that range, ignored the probability of the existence there of fossiliferous Cretaceous and Tertiary seas and lake-basins, in comparison with which those of the Rocky Mountains may sink into insignificance, both as to extent and productiveness. Professor Huxley has, indeed, suggested, as an alternative or escape, the possible former existence of a submerged continent, from which both Asia and America derived their types of animals and plants, which is tantamount to an opinion that the subject is not yet advanced enough for other than speculation.

Other results of Professor Marsh's labours are equally instructive —such, I mean, as support the doctrine of Evolution; but these have been made known to the scientific public of this country by Mr. Huxley, who examined the Yale College Museum last year. Since then, as I was informed by the Professor, during a visit to the same museum, his species and specimens have largely increased in number and proportionately in value—that is, from the palæontological point of view; and the address which I have quoted gives a summary of the state of the whole collection up to the present time.

A few words on the magnificent collection of vegetable remains, Cretaceous and others, that have been studied and described by Mr. Leo Lesquereux in various published Reports of the U. S. Geological Survey, and in separate works issued under its auspices, may be fitly spoken here. It would be difficult to overrate the value of these contributions to fossil Botany, which, in its present state of advancement, affords no results comparable with those obtained from the animal kingdom for fixing the limits of periods, tracing the direction of migrations and the areas of distribution, or for following the devious paths of evolution. In the whole range of the natural sciences no study is so difficult, and at the same time so fruitless, if we regard the amount of results accepted by botanists, as compared with the prodigious labour their acquisition by palæontologists has demanded. Of all the orders of fossil plants of the formations referred to, the Gymnosperms alone have, as a rule, yielded much trustworthy information; and this is due to their texture, to the peculiar character of their vegetative and reproductive organs, to the frequent adhesion of these to the branchlets, to their gregarious habits, to their wide distribution, and to their close affinity with existing species. Of other orders and genera of plants, with the exception of a few with well-characterized foliage, as the Palms, the identifications of a large proportion hitherto published are not recognized as having much claim to confidence by those who have the largest acquaintance with the varied forms of the vegetative organs of plants. And if the identification of the fossil leaves of one country is so hazardous, what must be the risk of identifying the fossil leaves of one continent with those of another? a forlorn hope which has constantly to be resorted to. The result, in the case of the North-American Cretaceous and Tertiary floras, has been the discovery of certain well-ascertained plants, which would appear to show that various prevalent existing American genera have inhabited that continent from a very early period; but that, along with them, there existed types of European, Asiatic, and Australian genera, temperate and tropical, that are no longer associated anywhere on the globe in a state of nature. It is well, under such perplexing conditions, that men of ability and unconquerable zeal (such as Heer, Saporta, and Lesquereux) are to be found who will undertake to investigate them; and while thanking them cordially for what they have done, I would urge upon them the importance of constant reference to large Herbaria, in order to enable them fully to appreciate the variability of foliar organs, and the deceptive nature of the characters they present.

Though doubtless the most productive to science generally, Dr. Hayden's is, I need hardly say, neither the oldest of the States' Surveys nor the first that brought its resources to bear on other matters than geography and geology. Indeed, from the beginning of the century, the Americans have busied themselves with inquiries into the resources and productions of their States—never on any recognized system, too often under difficulties and discouragements, not seldom to be nipped in the bud, or, worse still, sacrificed when the fruit was fit for gathering, through the ignorance or parsimony of the holders of the national purse; but, thanks to the single-mindedness of the labourers, never without some good, and often with great results. The Coast Surveys are admirable alike for their system, for their breadth of purpose, for the attainments and ability of the officers in charge of them, and for the minute topographic accuracy aimed at and attained—an accuracy which, I need not say, is unattainable by such surveys as that here briefly described. The various surveys for railways across the continent have contributed a very library to natural science in many departments; and some of the individual States have, through the like agency, contributed greatly to our knowledge of their natural history and other products. For an excellent and full account of the history, labours, and results of all these, I must refer you to Prof. Whitney's article on "Geographical and Geological Surveys" in the 'North American Review' for July and September 1875, which he was so kind as to send me at the moment of my departure from the States. Prof. Whitney's own Geological Survey of California and Nevada is one of the very best of the series. It was begun in 1864, and continued for ten years; but after the publication of a topographical map, and some very valuable results, including natural history, at a most moderate cost, the whole work was stopped by the State Legislature, and the geological maps and sections, though admirable and paid for, have consequently never been given to the public! The last of these Surveys which I shall mention is that of Kentucky by Professor Shaler, the State Surveyor, of which the first volume of the Report has just appeared, containing, besides articles on prehistoric remains, fossil Brachiopods, and caverns and cavern-life, an exhaustive article by Mr. Allen, of singular interest, on the Bisons of America, living and extinct.

The American Flora.—Though I have as yet little to say of the results of Dr. Gray's and my own investigations under the Survey, I have every reason to hope that, having been extended through the Sink, Salt, or desert regions west of the Rocky Mountains, and thence across the Sierra Nevada to the Pacific coast, they will, with the materials previously obtained by my fellow traveller and myself, enable us to correlate our several researches into the distribution of North-American plants, and to point out the lines along which the migrations of the existing types were directed, and the countries whence they migrated.

As regards the components of the United-States flora, these seemed to us to be threefold, and to be intermixed throughout the continent—an endemic American, a European, and an Asiatic: it seemed that the flora was a ternary compound, so to speak, while that of the temperate Old World was, in a continental point of view, binary—Europe and Asia having many types in common, but very few representatives of the strictly American flora. The distribution of North-American plants, unlike the European, is mainly in a meridional direction, the difference of the floras of the Eastern, Central, and Western States being wonderfully great—far greater than those of similarly situated regions in the Old World. The European components extend over the whole breadth of the continent, diminishing, however, to the westward. The American components present many localized genera, inhabiting the Eastern, Central, and Western States respectively; they increase in numbers and peculiarity, as also in restriction of range, towards the west. The Asiatic components are found both in the Eastern and Western States, but hardly at all in the Central; and some of them are common to both the east and west, while others are peculiar to each. But whereas the European components prevail on the side towards Europe, the maximum of Asiatic representation is on that remote from Asia. This has been conspicuously shown by Gray's discovery, in the Eastern States, of single representatives of Japanese genera previously supposed to be monotypic; and what is most noteworthy is, that such representatives are in some cases extremely rare and local plants, found in single and very restricted areas, indicating a dying-out of the Asiatic representation in America.

The evidences of climatic changes in past eras of the existing flora of the continent are seen in the prevalence of arctic and northern species of plants in the alpine zones of the meridional mountain-chains, the Appalachian, Rocky Mountains, and Sierra Nevada, even as far south as the 33rd parallel. These plants had spread southwards during a period of cold, and on its subsequent mitigation had retired to the lofty situations they now inhabit. To the former existence of a warmer climate we may partly look for the extension of Mexican types to the dry regions west of the Rocky Mountains up to the 41st parallel; and to it may be attributed the remarkable northward extension of the Cacti in a very narrow meridional belt, scarcely one hundred miles broad, along the eastern flanks of the same mountains, from their head-quarters in New Mexico, in the 33rd, almost to the 50th parallel.

Of existing influences that determine the development in amount of the vegetation of a country, and the extension in various directions of its components, none are so powerful as the distribution of rainfall and of vapour in the atmosphere. This subject will repay a careful study in America, especially in connexion with the presence or absence of woodlands and forests, an excellent map of which by Professor Brewer, of Newhaven, was published in 1873 by the Supreme Government, in which the density of the forests in each State is portrayed by five shades of colour.

I must not end my notices of some of the labours of our scientific brethren in the United States without expressing my admiration of the spirit and the manner in which the Government and people have cooperated in making known the physical and biological features of their country, and my conviction that the results they have given to the world are, whether for magnitude or importance, greater of their kind than have been accomplished within the same time by any people or government in the older continents. How great would now be our knowledge of the climate and natural features of India and of our Colonies had the excellent Trigonometrical Survey of the one and the territorial and Geological Surveys of the others been supplemented by Reports such as those to which I have directed attention!

On the motion of Mr. De La Rue, seconded by Sir James Alderson, it was resolved—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The President then proceeded to the presentation of the Medals.

The Copley Medal has been awarded to Professor James Dwight Dana, of Yale College, Newhaven, United States, for the numerous, varied, and important contributions to Mineralogy, Geology, and Zoology with which he has enriched science during more than fifty years. Professor Dana's first published paper bears the date of 1823, while the year 1877 finds him, as ever, vigorously at work.

Commencing his career with the inestimable advantage of a sound training in mathematics, physics, and chemistry, one of Professor Dana's earliest writings is an essay upon the connexion of electricity, heat, and magnetism. He then turned his attention to mineralogy; and, after exhibiting his thorough study of both the crystallographic and the chemical aspects of minerals by the publication of a large number of separate memoirs, he produced a systematic treatise on mineralogy, which at once took the place it still holds among standard works upon the subject.

In geology, the diversity and importance of Professor Dana's labours are not less remarkable. Not only have multitudinous detached essays, embodying the results of wide and accurate observations in all parts of the world, and on all classes of geological phenomena, proceeded from his pen, but his 'Manual of Geology,' of which a new edition appeared two years ago, is at once a most clear and comprehensive statement of the present state of geological science, and a complete, though necessarily condensed, monograph of the geology of North America; and, it may be added, few treatises on this branch of knowledge show so thorough and practical an acquaintance with all those sciences which are auxiliary to geology, or so extensive and profound a study of the phenomena presented by the existing condition of the globe, from the knowledge of which every rational attempt to reconstruct the past history of the earth, upon the data afforded by its rocks and their organic contents, must start.

As naturalist to the United States Exploring Expedition, which made a circumnavigatory voyage, under the command of Captain Wilkes, in the years 1838 to 1842, Professor Dana enjoyed unusual opportunities for zoological investigation; and his remarkable works on the Zoophytes and the Crustacea observed during the voyage testify to the admirable use which he made of those opportunities. Nor has Professor Dana confined himself to the merely descriptive side of zoology; but, drawing general conclusions from his vast store of accurate observations, he has published views on classification and on questions of general morphology of much originality and breadth of view.

The Medal was received for Prof. Dana by the Hon. Edwards Pierrepoint, United States Minister. The President, in delivering the Medal, expressed his assurance of the esteem and regard in which Prof. Dana was held by the Royal Society, not less for his own scientific achievements than for the liberal aid he has always rendered to other investigators.

A Royal Medal has been awarded to Mr. Frederick Augustus Abel, for his physico-chemical researches on gunpowder and explosive agents.

Mr. Abel's career as a contributor to chemistry commenced about 30 years ago. Between 1847 and 1865 he contributed a number of papers to the Chemical Society, which were published in their Journal: some of the investigations were made in conjunction with other chemists; among these were the action of nitric acid on cumol (1847), and researches on strychnine (1849), when the composition of that alkaloid was finally established. They were followed by papers relating to metallurgy (copper) and analytical processes, one of which, on the application of electricity to the explosions of mines, may have led to his various works on explosives, on which the claims of Mr. Abel for the distinction of a Royal Medal mainly rest. So far back as 1863 he directed his attention to the study of gun-cotton in consequence of the development of its manufacture in Austria for artillery purposes, and in that year communicated to the British Association a report on the preliminary results arrived at by his experiments on the Austrian process, and the products furnished by it.

In 1866 a memoir was sent to our Society, which was published in the

Phil. Trans. vol. clvi. p. 269, "On the Manufacture and Composition of Gun-cotton." In this paper, as the result of a long series of experiments, made with great accuracy, the conditions were laid down for its uniform manufacture and purification; and the true nature of gun-cotton (trinitro-cellulose) was finally established by an exhaustive series of analytical and synthetical experiments.

This paper was followed by another in 1867, published in the Philosophical Transactions, vol. clvii., entitled, "On the Stability of Guncotton," which was considered worthy of being made the Bakerian Lecture for that year. This memoir details the results of four years' extensive experiments on the effects of light and heat on gun-cotton, and upon the protective action of water at low and high temperatures. It will be recollected that the uncertain stability which had been characteristic of gun-cotton was conclusively traced to minute quantities of unstable substances remaining in the fibre, even after the most careful purification by the methods hitherto known, and the efficiency of simple measures for securing the stability of gun-cotton was established. This led ultimately to the development of a system of manufacture of gun-cotton which permitted of its ready manufacture in a high state of purity (pulping).

Mr. Abel did not, however, confine his attention to gun-cotton; and, indeed, in 1864 had sent in a paper to the Royal Society, which was published in the 'Proceedings,' vol. xiii., on "Some Phenomena exhibited by Gun-cotton and Gunpowder under special conditions," in which the behaviour of these substances when exposed to high temperatures in rarefied atmospheres and in different mechanical conditions was described.

In 1869 a memoir, entitled "Contributions to the History of Explosive Agents," was printed in the Philosophical Transactions, vol. clix. In this memoir is discussed the influence of more or less strong confinement and other mechanical conditions under which the *detonation* of such compounds and mixtures was developed. It will be recollected that some striking results were obtained in the examination of the behaviour of explosive compounds when exposed to *initiative* detonations of different character.

These phenomena were more fully discussed in a second memoir, published in the Philosophical Transactions for 1874, vol. clxiv.; it includes an exhaustive investigation of the transmission of detonation from one mass of gun-cotton, fulminates, and nitro-glycerine to other distinct masses in the open air, and also through the agency of tubes. The causes of interference with the transmission of detonation-force, and the development of detonation as distinguished from explosion, were clearly discussed. The influence of dilution by solids and by liquids on the susceptibility of explosives to detonation, and also the velocity with which detonation is transmitted by different explosive agents under various conditions, was carefully studied. Some important results were obtained by the comparison of the behaviour of the liquid nitro-glycerine and the solid pulped and compressed gun-cotton devised by Mr. Abel. Among other things, the detonation of gun-cutton when thoroughly saturated with water, the transmission of detonation to distinct masses of gun-cotton enclosed in receptacles in which the space between the masses was filled up with water, and, further, the value of water as a violent disruptive agent (as in shells) when it was caused to transmit the force generated by the detonation of very small quantities of gun-cotton, which it surrounded, were established.

The last memoir published in the Philosophical Transactions, on "Fired Gunpowder," is a joint production of Mr. Abel and Captain Noble; and as the merit of the investigation, which has occupied the authors for some years, is divided, I do not dwell particularly upon it, except as affording evidence of the continuity of Mr. Abel's researches in physico-chemistry, which places him at the head of all other workers in the line of research which has mainly engaged his attention, and which has been productive of practical results of the greatest importance to this country.

[The Medal was received by Mr. Abel.]

A Royal Medal has been awarded to Prof. Oswald Heer, of Zurich, for his numerous researches and writings on the Tertiary plants of Europe, of the North-Atlantic Islands, North Asia, and North America, and for his able generalizations respecting their affinities, their geological and climatic relations.

It is mainly to Prof. Heer's labours that we owe those great advances made of late in our knowledge of the Miocene, Pliocene, and Post-Pliocene floras of Central Europe, which establish upon broad but safe grounds the close analogy existing between the vegetation of these epochs and that of the present period in Eastern North America and Eastern Asia. To Prof. Heer also we are mainly indebted for the remarkable discovery that a rich and varied arboreous vegetation, strikingly similar to what now obtains in temperate and subtropical countries, once extended to the Arctic Circle and far beyond it—a fact of which no adequate explanation has been found, and the importance of which, in relation to all questions as to the former geological and geographical conditions of the northern hemisphere, cannot be overestimated.

Prof. Heer's youthful studies were directed to botany and entomology. His scientific authorship commenced in 1836; and the early bent of his mind towards the higher problems of natural science is evinced by one of his very first memoirs, being 'Sur la Géographie Botanique de la Suisse,' published in 1837. His earliest work on fossil plants was upon those of the Rhone valley, published in 1846, since which period he has been uninterruptedly and indefatigably engaged on the comparative study of recent and fossil plants and insects—describing and illustrating them with a completeness and exactitude that have been thoroughly appreciated by geologists and botanists, and appending to the systematic descriptions of them geological and climatic considerations, remarkable alike for their caution and significance. Amongst his numerous works his 'Flora fossilis Helvetiæ,' 'Flora Tertiaria Helvetiæ,' and 'Flora fossilis Arctica' are conspicuous examples of well-directed labour and great learning; while the number of his minor works on various branches of biology testify to a life spent in successful devotion to science.

During Prof. Heer's long and laborious career he has been conspicuous for the liberal aid he has given to other investigators, and for the disinterested spirit in which he has worked out the collections brought by the government and private expeditions of various European nations from the northern and arctic regions. In particular, we are beholden to him for the labour he has bestowed upon our own Arctic collections, made during the last fifteen years, from that of Belcher to that of Nares, and especially for his elaborate and exhaustive memoir on the Miocene flora of Bovey Tracey, published in the 'Philosophical Transactions,'—labours all the more praiseworthy from being, for some years past, pursued in a recumbent posture, to which grievous bodily ill-health has confined him.

The Medal was received for Prof. Heer by M. Henri Vernet, Consul-General for Switzerland, to whom the President acknowledged the Society's obligations to Prof. Heer for his elucidations of the Geology of England and of the Flora of the Bovey-Tracey Coalfield, published in the Philosophical Transactions; and on behalf of the Society expressed his hope that Prof. Heer might soon be restored to health.

For the Davy Medal, now for the first time awarded, Prof. Robert Wilhelm Bunsen and Gustav Robert Kirchhoff, both Foreign Members of the Society, in recognition of their researches and discoveries in spectrum-analysis, have been selected.

The method of spectrum-analysis, as established by these two eminent men, must rank among the most important extensions of our means of investigating the properties of matter. Before that discovery, the chemical constitution of matter was examined solely by the study of the changes which take place within the narrow range of cases of which we can grasp and weigh the substance under investigation; but the tests employed in spectrum-analysis have no necessary dependence upon the distance of the material from the observer. It has enabled us to see, not only further, but deeper; for, on the one hand, it has led to the detection of many of the chemical constituents of masses distant from our planet, and, on the other hand, it has enabled us to discover many constituents of terrestrial minerals which had escaped detection until our ordinary methods of analysis were guided by the more refined tests afforded by the spectrum-analysis.

The Statutes relating to the election of Council and Officers were then read, and Dr. Allman and Mr. Dunkin having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Sir Joseph Dalton Hooker, C.B., M.D., D.C.L., LL.D.

Treasurer.—William Spottiswoode, M.A., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Thomas Henry Huxley, LL.D.

Foreign Secretary.—Professor Alexander William Williamson, Ph.D.

Other Members of the Council.—Frederick A. Abel, C.B., V.P.C.S.; William Bowman, F.R.C.S.; Frederick J. Bramwell, M.I.C.E.; William B. Carpenter, C.B., M.D., D.C.L.; William Carruthers, F.L.S.; William Crookes, V.P.C.S.; Prof. P. Martin Duncan, M.B., P.G.S.; William Farr, M.D., D.C.L.; Prof. William H. Flower, F.R.C.S.; Prof. G. Carey Foster, B.A., F.C.S.; John Russell Hind, F.R.A.S.; Lord Rayleigh, M.A.; Vice-Admiral Sir G. H. Richards, C.B.; Prof. Henry J. Stephen Smith, M.A.; Prof. Balfour Stewart, M.A., LL.D.; Prof. Allen Thomson, M.D., F.R.S.E.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	Total.
November 30, 1876.	4	46	253	258	561
Elected			+ 8	+ 9	+ 17
Deceased		− 3	− 12	− 11	− 26
Since compounded ..			+ 3	− 3	
November 30, 1877.	4	43	252	253	552

Statement of Receipts and Expenditure during the Year ending November 30, 1877.

	£	s.	d.
Annual Contributions	1052	0	0
Admission Fees	180	0	0
Compositions	628	0	0
Sale £1200 Consols	1124	17	6
Rents	321	4	1
Dividends (exclusive of Trust Funds)	1561	0	6
Proceeds of Jodrell Fund	184	6	4
Sale of Transactions, Proceedings, &c.	748	2	6
Sundries	6	15	11
	£5806	6	10
Donations	500	0	0
Balances from 1876	150	18	11

	£	s.	d.	£	s.	d.
Salaries and Wages				1203	6	9
Illustrations and Paper for Report of Naturalists (Transit-of-Venus Expeditions)				120	1	7
Eclipse Expedition				127	6	6
The Scientific Catalogue				210	12	0
Books for the Library				240	6	8
Binding ditto				72	1	11
Printing Transactions, Part II. 1875, Parts I. & II. 1876, and Part I. 1877, and Separate Copies to Authors and Publisher	1109	7	4	3202	9	10
Ditto Proceedings, Nos. 170–182	573	17	1			
Ditto Miscellaneous	98	3	2			
Paper for Transactions and Proceedings	272	7	8			
Binding and Stitching ditto	131	15	11			
Engraving and Lithography	1016	18	8			
Soirée and Reception Expenses				54	12	10
Coal, Lighting, &c.	101	9	4	446	12	10
Office Expenses	51	3	10			
House Expenses	155	6	7			
Tea Expenses	19	7	11			
Fire Insurance	41	15	0			
Taxes	22	13	9			
Advertising	14	17	0			
Few and Co., Law Charges	12	6	6			
Postage, Parcels, and Petty Charges	25	10	11			
Mablethorpe Schools, Donation	2	2	0			
				£5677	10	11

Trust Funds.

	£	s.	d.	£	s.	d.
Donation Fund Dividends	187	12	6	599	8	7
Handley Fund	264	17	9			
Rumford Fund	68	16	6			
Wintringham Fund	35	11	0			
Copley Medal Fund	9	19	2			
Davy Medal Fund	32	11	8			
				£7056	14	4

	£	s.	d.	£	s.	d.
Donation Fund	260	3	6	445	12	4
Rumford Fund	138	1	5			
Wintringham Fund	35	12	6			
Copley Medal Fund	4	15	8			
Prof. W. C. Williamson, Bakerian Lecture	4	0	0			
Dr. Sanderson, Croonian Lecture	2	19	3			
Balance at Bank				923	4	0
Balances on hand, Catalogue and Petty Cash				10	7	1
				£7056	14	4

W. SPOTTISWOODE,
Treasurer.

Carrington Bequest.

	£	s.	d.
Consols	2000	0	0

	£	s.	d.
Consols transferred to Society's account	2000	0	0

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £136 per annum.
Estate at Acton, Middlesex (34 A. 2 R. 4¾ P.), £167 17*s.* 10*d.* per annum.
Fee Farm near Lewes, Sussex, rent £19 4*s.* per annum.
One fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
Stevenson Bequest. Chancery Dividend. One fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £486 3*s.* 4*d.* in 1876–77).
£19,898 2*s.* 5*d.* Reduced 3 per Cent. Annuities.
£31,647 10*s.* 11*d.* Consolidated Bank Annuities.
£403 9*s.* 8*d.* New 2½ per Cent. Stock.—Bakerian and Copley Medal Fund.
£6221 14*s.* 1*d.* New Threes.—Jodrell Fund.
£667 5*s.* 6*d.* India Fours.
£660 Madras Guaranteed 5 per Cent. Railway Stock.—Davy Medal Fund.
£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

Trust Funds. 1877.

Scientific Relief Fund.

	£	s.	d.
Investments up to July 1872, New 3 per Cent. Annuities	6328	11	2
„ „ Metropolitan 3½ Consols	100	0	0
	£6428	11	2

Dr.	£	s.	d.	*Cr.*	£	s.	d.
Balance	267	12	1	By Grants	230	0	0
Dividends	190	19	8	Balance	229	12	9
Donation	1	1	0				
	£459	12	9		£459	12	9

Donation Fund.

£6333 10*s.* 4*d.* Consols.

	£	s.	d.		£	s.	d.
To Balance	318	13	7	By Grants	260	3	6
Dividends	187	12	6	Balance	246	2	7
	£506	6	1		£506	6	1

£2332 19*s.* Consols.

	£ s. d.		£ s. d.
Two years' Dividends, 1876	138 1 5	By Medals	59 10 0
		P. J. C. Janssen	78 11 5
			£138 1 5
Dividend, 1877	68 16 6		

Bakerian and Copley Medal Fund.

£403 9*s.* 8*d.* New 2½ per Cent.

	£ s. d.		£ s. d.
To Balance	71 16 7	By Gold Medal	4 15 8
Dividends	9 19 2	Bakerian Lecture	4 0 0
		Balance	73 0 1
	£81 15 9		£81 15 9

Wintringham Fund.

£1200 Consols.

	£ s. d.		£ s. d.
To Balance, 1876	35 12 6	By Payment to Foundling Hospital, 1877	35 12 6
Dividends, 1877	35 11 0		

Croonian Lecture Fund.

	£ s. d.		£ s. d.
To one fifth of Rent of Estate at Lambeth Hill, payable by the College of Physicians	2 19 3	By Croonian Lecture	2 19 3

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.
To Balance	213	11	9
Dividends	32	11	8
	£246	3	5

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

	£	s.	d.		£	s.	d.
To Balance	236	0	0	By Payments to Kew Committee	497	17	4
Dividends	497	17	4	Balance	236	0	0
	£733	17	4		£733	17	4

The Jodrell Fund.

£6221 14*s.* 1*d.* New 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Interest and Dividend	184	6	4	By Payment to Royal Society	184	6	4

The Handley Fund.

£5898 2*s.* 5*d.* Reduced 3 per Cent. Stock.

	£	s.	d.
Balance	231	4	3
Dividends	174	14	8
	£405	18	11

Account of Grants from the Donation Fund in 1876–77.

W. Huggins, for a new Clock for the Society's Telescope..	£50	0	0
W. Huggins, for Apparatus for carrying on Spectroscopic Researches with the Society's Telescope..........	100	0	0
Dr. Gowers, for Apparatus for continuation of Researches on the Action of the Sphincter Ani..............	20	0	0
W. R. Birt, for portable Stand for a Telescope belonging to the Society..................................	10	3	6
Rev. Dr. Robinson, for continuation of Experiments on the Theory of the Cup Anemometer	80	0	0
	£260	3	6

Account of the appropriation of the sum of £1000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XXV. p. 369).

1877.

1. Prof. W. C. Williamson, for continuation of his Investigations into the Organization of the Fossil Plants of the Coal-measures	£50
2. J. N. Lockyer, for continuation of Spectroscopic Researches.	200
3. Prof. B. Stewart, for an Analysis of the Kew Magnetical and Meteorological Records	100
4. G. J. Romanes, for continuing his Investigation on the Nervous System of the *Medusæ*	50
5. Dr. H. C. Bastian, for continuing his Researches into the conditions which favour or retard Fermentation in boiled and guarded fluids	25
6. Prof. G. F. Yeo, for an Investigation into the connexion between Latent Excitation and Contraction in Muscles	30
7. Prof. Rupert Jones, for preparing Drawings of Fossil Entomostraca	25
8. Prof. Maxwell Simpson, for a Research on the Synthesis of Organic Compounds......................................	100
9. T. Carnelly, for a Research on High Melting-points	60
10. Dr. J. E. Reynolds, for an Investigation of Alcohols from Flint	50
Carried forward..................	£690

Brought forward	£690
11. Dr. W. A. Tilden, for continuing his Researches into the constitution of the Terpenes and their Nitroso-substitution Compounds	30
12. Dr. Stenhouse, for continuing his Researches on Organic Chemistry	100
13. Profs. Roscoe and Stewart, for carrying out an extended Map of the Solar Spectrum, in conjunction with Mr. Lockyer	100
14. Prof. J. Dewar, for continuing his Researches on the Picoline and Chinoline Bases	100
15. Prof. G. D. Liveing, for continuing a Research on the Occurrence and Causes of Multiple Spectra of Elementary Bodies	50
	£1070

Dr.

		£	s.	d.
To Balance on hand, Nov. 30, 1876		1016	0	10
Grant from Treasury, 1877		1000	0	0
Repayments.				
Prof. Cayley, balance,	£24 18 10			
Mr. Scott, „	16 4 0			
Prof. Stewart, „	32 11 4			
		73	14	2
Interest		3	6	0
		2093	1	0

Cr.

	£	s.	d.
By Appropriations, as above	1070	0	0
Printing, Postage, and Advertising	7	7	6
Balance on hand, Nov. 30, 1877	1015	13	6
	2093	1	0

Account of appropriations from the Government Fund of £4000 made by the Lords of the Committee of Council on Education, on the recommendation of the Council of the Royal Society.

1877.

J. Kerr, for aid in Electro-Optic and Magneto-Optic Researches	£200
J. E. H. Gordon, for Experimental Measurements of the Specific Inductive Capacity of Dielectrics	50
Prof. Guthrie, for Apparatus and Assistance in (1) the Determination of the Latent Heats of the Cryohydrates, and the Vapour	
Carried forward	£250

Brought forward	£250
Tensions of Colloids; and (2) the Examination of Heat Spectra and Radiant Heat by means of varying Electrical Resistance in Thin Wires	150
J. T. Bottomley, to aid in carrying out a series of Experiments for determining the Conductivity for Heat of various Liquids and Solutions of Salts	100
Sir William Thomson, for Assistance and Materials for a continuation of Experiments on the Effects of Stress in Magnetism	100
J. A. Broun, for payment of Computers and other Expenses attending correction of the Errors in the published Observations of the Colonial Magnetic Observatories	150
W. Crookes, for assistance in continuing his Researches connected with "Repulsion resulting from Radiation"	300
Prof. Rücker and Prof. Thorpe, for a Comparison of the Air and Mercurial Thermometers	50
F. D. Brown, for an Investigation of the Physical Properties, the Specific Gravity, Expansion by Heat and Vapour Tension, of the Homologous and Isomeric Liquids of the C_nH_{2n+1} Series	100
Prof. Roscoe, for continuation and extension of the Experiments on the Self-registering Method of measuring the Chemical Action of Light	100
Sir William Thomson, for Investigation and Analysis of Tidal Observations and Periodic Changes of Sea-level	200
Dr. Joule, for Experimental Investigations into the Mechanical Equivalent of Heat	200
Dr. I. B. Balfour, for the expense of Illustrations for a 'Monograph of the Pandanaceæ,' which he is now working out	50
Prof. Parker, for assistance in Researches on the Morphology of the Vertebrate Skeleton and the Relations of the Nervous to the Skeletal Structure, chiefly in the Head	300
Rev. W. H. Dallinger, for Microscopic Investigations of Monads, Bacteria, and other low forms of Life	100
H. T. Stainton, for aid in publishing the 'Zoological Record'	100
Prof. M'Kendrick, for Apparatus for a Research into the Respiration of Fishes	75
Prof. Gamgee, for a more complete Survey than has yet been made of the Physiological Action of the Chemical Elements and their more simple Compounds, with the object, in the first instance, of establishing a Physiological Classification of the Elementary Bodies	50
Rev. J. F. Blake, for aid in compiling and publishing a 'Synopsis of the British Fossil Cephalopoda'	100
Carried forward	£2475

Brought forward	£2475
Prof. A. H. Garrod, for aid in preparing for publication an exhaustive Treatise on the Anatomy of Birds	100
Dr. Brunton, for Researches into the Physiological Action of the most important compounds of Nitrogen, and into the Action of certain Poisons, and for Apparatus	80
Dr. Murie (per Zoological Society), for aid in completing and publishing Three Memoirs:—Anatomy of the Kingfishers, 4to, with 5 plates; On Extinct Sirenia, 4to, with 6 plates; Osteology of the Birds of Paradise, fol., 3 plates	150
E. A. Schäfer, to pay the wages of an Assistant to give mechanical aid in Histological and Embryological Research	50
H. Woodward, for continuation of work on the Fossil Crustacea, especially with reference to the *Trilobita* and other extinct Forms, and their publication in the Volumes of the Palæontographical Society	100
Dr. Burdon Sanderson, for an Investigation of the Normal Relation between the activity of the Heat-producing Processes and the Temperature of the Body	70
Prof. Schorlemmer, for continuation of Researches into (1) the Normal Paraffins, (2) Suberone, (3) Aurin	300
Dr. Armstrong, for continuation of Researches into the Phenol Series, and into the effect of Nitric Acid on Metals	300
Profs. King and Rowney, for Researches to determine the Structural, Chemical, and Mineralogical Characters of a certain Group of Crystalline Rocks represented by Ophite	60
W. J. Harrison, towards the expense of Collecting and Describing Specimens of the Rocks of Charnwood Forest	50
W. N. Hartley, for Researches into the Photographic Spectra of Organic Substances—into the Phosphates of Cerium—the conditions under which liquid Carbonic Acid is found in Rocks and Minerals—the Double Salts of Cobalt and Nickel, and for other Investigations, and for Assistance	100
Dr. Burghardt, for a Research into the Origin of the Ores of Copper and (if possible) of Lead, their mode of Formation, and the Chemical Connexion (if any) between the Ore and its Matrix	50
Prof. Church, for a Research into the Colouring-matters of Colein, of Red Beet, and for the Study of Plant Chemistry	50
	£3935

Printing, Advertising, Books, and Working-charges, £95 1*s.* 6*d.*

Report of the Kew Committee for the Year ending October 31, 1877.

The Committee would commence their Report with the expression of their heartfelt regret at the decease of Mr. J. P. Gassiot, who had been, since 1851, among the most active Members of the successive Kew Committees, first of the British Association, and latterly of the Royal Society. Mr. Gassiot had not only devoted freely to the management of Kew Observatory a large portion of his valuable time, but also had, in 1871, most munificently endowed the establishment with the sum of £10,000 Italian Irrigation Stock, for the purpose of maintaining in a state of thorough efficiency the self-recording observations in terrestrial magnetism and in meteorology.

Committee.—The Committee is constituted as follows :—

General Sir E. Sabine, K.C.B., *Chairman.*

Mr. De La Rue, *Vice-Chairman.*
Capt. Evans.
Mr. F. Galton.
Vice-Adm. Sir G. H. Richards.
The Earl of Rosse.
Mr. R. H. Scott.
Lieut.-General W. J. Smythe.
Lieut.-General Strachey.
Mr. E. Walker.

Mr. R. H. Scott, at his own request, has been for the future relieved of the duties of Hon. Secretary, which he has performed ever since the Observatory came under the management of the Royal Society, and the duties of that post will be in future discharged by the Superintendent.

Magnetic Work.—The Magnetographs have been in constant operation throughout the year. The declination instrument was slightly deranged for a short period by the presence of some extremely minute organic fibres between the mirrors. In accordance with the usual practice, determinations of the scale-values of all the instruments were made in the first week of the new year.

The inconvenience occasionally experienced by the dropping, on to the Magnetographs, of water condensed in the smoke-tubes over the gas-burners has been obviated by the attachment of suitable catch-bottles.

The monthly observations with the absolute instruments have been continued as usual by Mr. Figg, and the results are given in the Tables appended to this Report. A re-determination of the moment of inertia of the magnet KC 1 was made in October; the value accorded with the previously found amount.

A paper "On the Temperature-correction and Induction-coefficients of Magnets," as determined in the results of various experiments at the Observatory, was read before the Royal Society, and printed in vol. xxvi. of the 'Proceedings.'

Sir Edward Sabine having brought the discussion of the Magnetical Observations carried on under his superintendence to a close in a final "Contribution," presented to the Royal Society (No. XV., Phil. Trans. vol. 167), represented to the War Office that he was able to dispense with the further services of the two Sergeants of the Royal Artillery who had acted as his clerks. These men were in consequence withdrawn on the 31st of March.

The documents deposited in Sir E. Sabine's late office have been presented by the "War Office" to the "Royal Society," and, in conformity with instructions received from the Council, will be retained in the custody of the Observatory. A detailed list of these documents and papers is now in course of preparation.

The tabulation of the magnetic curves, which was suspended at the time of writing the last Report, has been resumed, and Tables of the Declination have been completed as far as December 31st, 1876.

At the request of Prof. Balfour Stewart, the range of the declination-needle has been determined for every day during the years 1858–73 inclusive. The results have been discussed by him in a paper read before the Royal Society, March 22, 1877, entitled, "On the Variations of the Daily Range of the Magnetic Declination as recorded at the Kew Observatory," Proc. Roy. Soc. vol. xxvi.

In addition, magnetic data have been supplied to Dr. Atkinson, Prof. M'Leod, the Hydrographic Department, Messrs. Gee, Grove, and Groenman.

The officers attached to the late Arctic Expedition have visited the Observatory, and made several sets of observations with the instruments employed by them on their voyage, Kew being the base station to which all their observations are referred.

The Committee have received, with great satisfaction, a communication from Mr. R. Ellery, F.R.S., of the Melbourne Observatory, in which he states that, in deference to the opinions elicited by the circular of the Kew Committee from the chief authorities on terrestrial magnetism, as explained in last Report, the Government of Victoria has furnished funds for the erection of a new Magnetic Observatory at Melbourne.

Meteorological Work.—The several self-recording instruments for the continuous registration respectively of pressure, temperature, humidity, wind (direction and velocity), and rain have been maintained in regular operation under the care of Mr. T. W. Baker, assisted by T. Gunter. The daily standard eye-observations for the control of the automatic records have been made regularly, as well as daily observations in connexion with the Washington synchronous system.

The Observatories of the Meteorological Council at Aberdeen, Armagh, Falmouth, Glasgow, Stonyhurst, and Valencia have been visited by Mr. Whipple, and their instruments inspected.

The examination and checking of the work of the self-recording Observatories of the Meteorological Committee was discontinued in November 1876, and the scales and appliances used in the work returned to the Meteorological Office. Mr. Aldridge, an assistant engaged in the work, accepted an appointment from the Meteorological Committee. Mr. Harrison is employed at Kew, subject to the condition that he may be at any time sent to any of the Observatories to relieve the officer in charge in case of illness or other cause, if required by the Meteorological Council to do so. The tabulation of the Kew meteorological traces is performed by Mr. Hawkesworth.

This change of arrangements has involved a considerable reduction in the amount allowed by the Meteorological Office to Kew, as its central Observatory. This sum is now £150, instead of £400, as heretofore.

Electrograph.—The cause of derangement of this instrument having been apparently removed, by dismounting and thoroughly cleaning it, its action has been fairly satisfactory throughout the year.

Considerable difficulty has been experienced at times in maintaining the cistern of the water-dropping collector in a sufficiently insulated condition. The substitution of a set of Varley's telegraph insulators for the ebonite supports previously used has been attended with good results. The tabulation of the curves obtained from this instrument has not yet been commenced.

An instrument having been constructed for the Royal Observatory, Greenwich, similar to the one at Kew, certain minor alterations in the details have been introduced which Mr. Whipple's experience has suggested.

Photoheliograph.—The re-examination of the measurements of the Kew sun-pictures, as noticed in former Reports, has been steadily carried on throughout the year by Mr. Whipple, assisted by Mr. M'Laughlin, who has been temporarily engaged for this purpose.

The rate of progress has been somewhat slow during the past year, owing to the enormous number of spots visible on the sun's surface during the (maximum) year (1870) now under discussion. On several days, recently measured, more than 150 spots have had their positions determined. Mr. Marth is still engaged on the reduction to heliocentric elements.

All of these operations have been conducted at the expense of Mr. De La Rue.

The eye-observations of the sun, after the method of Hofrath Schwabe, have been made daily by Mr. Harrison, when possible, as described in the Report for 1872, in order, for the present, to maintain the continuity of the Kew record of sun-spots.

Extra Observations.—The Solar-radiation Thermometers are still observed daily.

The Campbell Sundial, described in the 1875 Report, continues in action, and the improved form of the instrument, giving a separate record for every day of the duration of sunshine, has been regularly worked throughout the year and its curves tabulated.

Wind Component Integrator.—The Wind Component Integrator, an instrument which automatically resolves the velocity of the wind into its rectangular components, which was exhibited by Prof. von Oettingen, of Dorpat, at the recent Loan Exhibition, having been lent by the Guarantors of that Exhibition to the Meteorological Council, has been erected in the sun-room of the Observatory at their expense. The instrument has been temporarily attached to the existing anemograph, and its gearing so modified as to enable it to print from type-wheels on a strip of paper the number of miles of wind passing over the building from the north, east, south, and west during every half-hour *.

At the request of the Editor of the 'Times,' a copy of the traces of the self-recording instruments on a reduced scale, together with an epitome of the general features of the weather, is now prepared. This is published every week in that journal, the cost to the Observatory being defrayed by the proprietors.

Verifications.—This branch of the work of the Observatory has been carried on with considerable success as usual. The following magnetic instruments have been verified :—

A set of Magnetographs for the Batavia Observatory.
" " " Potsdam Observatory.
A Collimator Magnet for Dr. van der Stok.
A Dip-circle, with extra needles, for Vice-Admiral Sir Charles Shadwell.
Two Azimuth Compasses for Mr. Frewen.

There have also been purchased on commission and verified :—a Unifilar for Dr. Da Souza, Coimbra; a Collimator magnet for Mr. Kingston, Toronto; a Dip-circle for Prof. Weihrauch, Dorpat; a Dip-circle for Prof. Houzeau, Brussels Observatory; and two Dip-needles for Prof. Smirnow, Kazan. There are also now undergoing verification a set of Magnetographs for the Brussels Observatory, and a Unifilar and Dip-circle for the Marine Observatory, San Fernando, Spain.

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* A complete description of this instrument, illustrated in detail, is to be found in the 'Repertorium für Meteorologie,' vol. v. No. 10, St. Petersburg, 1877.

The following meteorological instruments have been verified, this portion of the work being entrusted to Mr. T. W. Baker, assisted by Messrs. Foster, Constable, and Gunter:—

Barometers, Standard	71
„ Marine and Station	113
	184
Aneroids	25
Thermometers, ordinary Meteorological	1428
„ Boiling-point Standards	57
„ Mountain	22
„ Clinical	2281
„ Solar radiation	75
	3863

In addition, 102 Thermometers have been tested at the melting-point of mercury. 12 Standard Thermometers have been calibrated and divided at Kew.

The following is the list of miscellaneous instruments which have been verified:—

Hydrometers	249
Rain-gauges	11
Dial Anemometers (Robinson's)	5

In addition to those for the Admiralty, the Meteorological Council, and the opticians, a number of instruments of various kinds have been verified for the Standards Department and the Ordnance Department, Woolwich.

The total increase in the number of instruments verified over last year has been 680, and in fees earned £25 11*s.* 2*d.*

There are now at the Observatory undergoing verification 56 Thermometers, 1 Hydrometer, and 35 Barometers.

London Office for receipt of instruments for verification.—Arrangements have been made with Mr. Strachan, of the Meteorological Office, who now receives instruments for verification at Kew, at 116 Victoria Street, Westminster, and takes charge of them on their return.

Mr. Galton's apparatus for testing Thermometers has continued to work satisfactorily; and a paper containing a description of the instrument, with an illustration, has been published in vol. xxvi. of the 'Proceedings of the Royal Society.'

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November 22, 1877.

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December 6, 1877.

Sir JOSEPH HOOKER, C.B., President, in the Chair.

The President announced that he had appointed as Vice-Presidents:—

The Treasurer.
Mr. Abel.
Dr. Farr.
Prof. Henry Smith.
Dr. Allen Thomson.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Tides at Malta." By Sir G. B. Airy, K.C.B., Astronomer Royal. Received July 14, 1877.

(Abstract.)

A self-registering tide-gauge had been erected in the harbour of La Valetta, and records had been made with it continuously from 1871, March 31, to April 28, and with interruptions through other days, under the care of Admiral Sir Cooper Key, Naval Commander at Malta; and the register sheets were placed in the hands of the Astronomer Royal. They form a band about 60 feet long.

The first step required to make the observations available was to eliminate the effects of frequent non-tidal undulations, whose period is approximately 21^m. This was done by carefully bisecting the intervals between adjacent elevations and depressions in about 1100 instances, and drawing a pencilled curve through the points of bisection.

It then became necessary to consider the best plan of reduction. It was determined to commence by comparison, in respect of times, with the predicted tides at London, as given in the Admiralty Tide-Tables, which are generally recognized as accurate, and are free from diurnal tide. The times of London high-water were therefore laid down on the register-sheets (without regard to difference of longitude); each tidal day was divided into 48 equal parts, and for each dividing point the ordinate of the tidal curve was measured. The number of measures is about 1900. These measures were collected in groups of 6 measures for each group (or 8 groups for each tidal day), and the mean of each group is exhibited in a Table.

The method of treating these means of group-measures is then ex-

plained. The object is to express every height on each separate day by the following formulæ, in which θ is the tidal angle, increasing from 0 at the beginning to 2π, or 24^h, at the end of the tidal day:—

For mean height, M;
For semidiurnal tide, $P.\sin 2\theta + Q.\cos 2\theta$;
For diurnal tide, $p.\sin \theta + q.\cos \theta$.

The mathematical process is investigated, and an easy practical rule is found for its application. Thus the numerical values of M, P, Q, p, q are found for every day. The two tidal expressions are without difficulty converted into the following—

Half semidiurnal range × cosine of ($2\theta - 2$ retard of semidiurnal high-water on the zero of tidal time),

Half diurnal range × cosine of (θ—retard of diurnal high-water on the zero of tidal time),

By the formulæ,

Half-range $= \sqrt{(P^2+Q^2)}$ or $= \sqrt{(p^2+q^2)}$ for the two tides respectively,

Tan 2 retard for semidiurnal tide $= \frac{P}{Q}$,

Tan retard for diurnal tide $= \frac{p}{q}$.

Thus the retard of each tide on the zero of tidal time is found for every day. The adopted zero of tidal time, for a reason given in the paper, is 16^m earlier than the tabular time of London high-water: and thus the real time of each of the Malta high waters for every day is found. This is then compared with the time of moon's transit in the Nautical Almanac; and thus the retard of each high water on the moon's transit is found. For more distinct view of the changes the lunation is divided into eight equal parts, and the means of the several classes of results are taken for each eighth part, by a process explained in the paper.

The following are the principal results:—

1. The value of M, the mean height for each day, has a regular and well-defined luni-menstrual change connected with the moon's declination. It is suggested that, viewing the extreme slowness of the change, it is probable that this inequality differs little in magnitude and epoch from the corresponding inequality on the oceanic shores of Spain and Morocco, and that perhaps it gives the best measure of that inequality.

2. The semimenstrual inequality in time of the semidiurnal tides is very well marked. In magnitude it is sensibly the same as that at London, but its epoch is about three days earlier.

3. The semimenstrual inequality in height of the semidiurnal tides is also very well marked. Its epoch is earlier than that for London by about three days; but the proportion of its coefficient in height to the coefficient of semidiurnal tide in height is greater than at London.

(It is curious that these results should be deduced with such certainty and accuracy from tides in which the greatest single coefficient is 3¾ inches.)

4. The mean retard of semidiurnal high water on the moon's transit is about $16^h\ 4^m$, which may be taken as the establishment at Malta.

5. The results for the diurnal tides (whose coefficient in the mean is about 0·3 inch) are somewhat discordant, but appear to exhibit a periodical law of coefficients, such as is proper for them.

The last point for examination is, the character of the non-tidal undulations already mentioned. These, in many cases, far exceed the tidal oscillations. Similar undulations at Swansea had attracted attention many years ago ('Encyclopædia Metropolitana,' "Tides and Waves"; and private correspondence with J. W. G. Gutch, Esq.). Lately, however, the undulations of the same character in the Swiss lakes, called *Seiches*, have been carefully examined by Dr. Forel; and there appears to be no room for doubt that the non-tidal oscillations at Malta are genuine *Seiches*. They appear to be formed by waves reflected from opposite shores, producing stationary waves between them. The shores concerned in forming the *Seiches* of Malta seem to be those of Africa and Sicily. A Table, exhibiting the principal elements of their intervals and their magnitudes, is given in the paper.

II. "Observations on Hermetically-sealed Flasks opened on the Alps." In a Letter to Professor Huxley, Sec. R.S. By Professor Tyndall, LL.D., F.R.S. Received September 21, 1877.

Alp Lusgen, 18th September, 1877.

My Dear Huxley,—Though the question of "Spontaneous Generation" is, I believe, practically set at rest for the scientific world, you may possibly deem the following facts of sufficient interest to be communicated to the Royal Society.

I brought with me this year to the Alps sixty hermetically-sealed flasks, containing infusions of beef, mutton, turnip, and cucumber, which had been boiled for five minutes in London and sealed during ebullition. They were packed in sawdust, and when opened at the Bel-alp the drawn-out and sealed ends of six of them were found broken off. These six flasks were filled with organisms, the remaining ones were pellucid and free from life.

Two or three of them were subsequently broken by accident, but for six weeks fifty of the flasks remained perfectly clear.

At the end of this time I took twenty-three of them into a shed containing some fresh hay, and there snipped off their sealed ends with a pair of pliers. The air of the hay-loft entered to fill the vacuum produced by the boiling in London. Twenty-seven other flasks were taken immediately afterwards to the edge of a declivity, which might almost be called a precipice, with a fall of about a thousand feet. A gentle breeze was blowing from the mountains, which were partly snow-covered, and partly of bare rock, towards the precipice. Taking care to cleanse my pliers in the flame of a spirit-lamp, and to keep my body to leeward of the flasks, I snipped off their sealed ends.

The two groups of flasks were then placed in our own little kitchen, where the temperature varied from about 65° to 90° Fahrenheit.

Result:—Twenty-one of the twenty-three flasks opened on the hay-loft are filled with organisms; two of them remain clear.

All the flasks opened on the edge of the precipice remain as clear as distilled water. Not one of them has given way.

This is a striking confirmation of the experiments of Pasteur upon the Mer de Glace.

Ever, my dear Huxley,
Yours faithfully,
JOHN TYNDALL.

III. "Researches on the Effect of Light upon *Bacteria* and other Organisms." By ARTHUR DOWNES, M.D., S.Sc. Cert. Cantab., and THOS. P. BLUNT, M.A. (Oxon.), F.C.S. Communicated by J. MARSHALL, F.R.S., Professor of Anatomy to the Royal Academy of Arts. Received October 18, 1877.

The investigation to which the following communication relates was undertaken by us with the view of ascertaining, first, whether light could be shown to exert any appreciable influence, favourable or the reverse, upon the development of *Bacteria* and other organisms in certain of those solutions which afford a suitable medium for their appearance and increase.

We feel justified in thus presenting our earlier researches and the conclusions drawn from them, by considering that every fact, however small, which tends to throw light upon the life-history of these organisms is of importance as bearing upon questions of the highest moment and most varied interest.

In the experiments about to be recorded the contents of the tubes were in most cases examined under a high power, and the turbidity, when such occurred, was invariably found to be occasioned by swarms of *Bacteria*. The best index of the development of the *Bacteria* we found to be the degree of turbidity and the time of its commencement.

Obs. 1. *April* 24.—Eight ordinary thin test-tubes were cleansed with strong sulphuric acid and thoroughly rinsed with tap-water. They were then partially filled with freshly made unboiled Pasteur's solution, the exact composition of which is given in the Appendix (*A*). Four of the tubes were encased in thin sheet-lead so as entirely to exclude light, and four were left quite bare. The tops of all were loosely covered with sheet-lead capsules, and the whole were placed in a test-tube-stand outside a window facing south-east, and about thirty feet above the ground. None of the tubes were plugged.

May 4.—Tubes examined as to turbidity. The solution remains perfectly clear in the four bare tubes, while in each of the encased it has become distinctly and uniformly milky. This turbidity was proved microscopically to be caused by innumerable *Bacteria.* The bare tubes remained quite clear till May 28th, when they were unfortunately accidentally lost.

This observation was again and again repeated with similar result.

In the large majority of cases the exposed tubes remained clear for an indefinite time, and in every instance were conserved for a distinct period after their encased companions had become turbid. The most marked differences in the conduct of the two sets of tubes were obtained when the sun shone brightly; when for a period of a day or two at the commencement of the experiment the weather was close and sultry and the sky dull, the conservative effect of light appeared to be less pronounced. Thus, in an observation started on June 12, it was found on the 14th that while there was a thick zoogl&oelig;a and advanced turbidity at the upper part of the solution in the encased tubes, there had already commenced a much slighter but recognizable cloudiness in two tubes filled with the same Pasteur's solution, but freely exposed to the light. This result we attributed to the fact that throughout the whole of the 12th and 13th there was not one ray of direct sunlight, the sky being completely overcast and the atmosphere remarkably thick and hazy.

Obs. 2. *May* 5.—Two of the bare tubes used in Observation 1 were taken and the contents were found to be equally and perfectly clear. One was then encased, the other left as before, and both again replaced on the window-ledge for exposure.

May 16.—the contents of the encased tube are now distinctly turbid, those of the bare tube being perfectly clear. (The latter remained clear till May 28th, when it was destroyed.)

From this observation it is evident that the fitness of the cultivation-fluid as a nidus for the development of *Bacteria* is not impaired by the action of light; for we find that the contents of a tube, which remain perfectly clear so long as they are freely exposed to the sun's rays, swarm with *Bacteria* after being deprived of the access of light. This being so, it becomes important to determine whether light may exert, either directly or indirectly, a destructive influence on *Bacteria.* The obvious mode of settling this question would be to protect the tube to be insolated from subsequent impregnation, and finally to encase it. If then it remained clear for an indefinite period it might be fairly inferred that the *Bacteria* had been destroyed, either in their rudimentary condition, or successively as they came to maturity.

Obs. 3. *May* 30.—Two tubes were partially filled with Pasteur's solution (Appendix, *B*). Both were plugged with cotton wool. One was encased in paper, the other left bare.

June 5.—The encased tube was quite turbid.

June 12.—The bare tube, which had remained perfectly clear, was now encased, and continued quite free from turbidity up to June 21.

June 28.—Solution quite clear, but small tuft of mycelium growing at bottom.

July 7.—Examined with a high power. Mass of matted mycelium; no *Bacteria* or other living organisms seen.

July 11.—The solution now teems with *Bacteria*, having doubtless been impregnated by the dipping-rod used on July 7.

Obs. 4. *May* 30.—Two tubes containing unboiled Pasteur solution (Appendix, *A*) were plugged with cotton wool, capsuled, and insolated until June 21, when both were encased in the way previously described, the plug being withdrawn from one, and the lead capsule replaced.

Both tubes remaining clear up to July 2, the unplugged one was impregnated by means of a glass rod dipped in a solution containing abundant *Bacteria.*

July 4.—The impregnated solution is distinctly turbid. The plugged tube has remained perfectly clear up to the present date [October 11th].

From these observations we conclude that so far as *Bacteria* are concerned the solution may be absolutely and perfectly sterilized by sunlight. It is important to note, however, that in Observation 3 the germs of a fungus had apparently survived an amount of insolation which was fatal to the development of *Bacteria.* This interesting point will be further investigated in the sequel.

Obs. 5. *May* 5.—Two of the tubes used in Observation 1 were taken; the contents of the one (encased) were turbid, and the other (bare) perfectly clear. The contents were well mixed, and divided between the two. They were then exposed as before.

May 8.—The contents of the encased tube are much the more turbid, those of the bare tube are but slightly, if at all, more turbid than on May 5th.

This experiment tends to indicate that not only is light inimical to the original development of the individual, but also materially retards the rate of increase, even when the organisms are present in a matured condition. The next observation was designed as a crucial test of this.

Obs. 6. *July* 10.—Seven tubes containing Pasteur's solution (A) were inoculated with a glass rod dipped in a solution teeming with *Bacteria*, care, however, being taken not to impair the translucency of that in the tubes in question. Six of these tubes were then insolated, the seventh being encased. The encased tube became turbid on July 14, the rest remained perfectly clear.

Our next object was to determine what period of exposure to light might be sufficient to sterilize a given solution.

Obs. 7. *July* 10.—Seven full-sized test-tubes were partially filled with solution A and plugged with cotton wool. Six were exposed to the light for periods varying from nine hours to seven days, and were then encased. There was but little direct sunlight upon them during this period, the tube exposed for nine hours having 3½ hours' sun, and the one encased at the end of seven days receiving in the aggregate about 12 hours of direct sunlight.

All, however, were sterilized, while the seventh tube, which was encased from the first, and served as a control, became cloudy with *Bacteria* on July 14th.

Obs. 8.—On July 29th, a very hot day, with much sunshine, six tubes (series *a*) containing solution A were each inoculated with two drops of a similar solution, which was swarming with *Bacteria.* They were all then plugged with cotton wool, exposed to the light for periods varying from thirty minutes to eleven hours, and then encased, the duration of sunlight received in each case being carefully noted.

On July 31st four, which had received half, one, two, and three hours' exposure respectively, were turbid with *Bacteria.* The other two, however, which had been exposed, the one for five and the other for eleven hours, were clear at this date; but some days later the former contained some *Bacteria* and a tuft of mycelium with sporidia, while the latter showed a similar growth of mycelium but no *Bacteria.* These tubes had received four and a half and nine hours' direct and powerful sunlight respectively.

On the same day six tubes (series *b*) containing the same solution, but not inoculated, were similarly plugged, insolated, and encased. These tubes all contained countless *Bacteria* on August 2nd. This somewhat dissimilar result we attribute to the fact that for series *a* we purposely chose very narrow tubes, not exceeding a third of an inch in diameter, while those of series *b* were about two thirds of an inch. The fact, however, that, while in Observation 7 we succeeded in sterilizing a solution by an exposure of nine hours, only three and a half hours of which were direct sunlight, nevertheless in Observation 8 (series *b*) we find the same solution breaking down after eleven hours, nine of which were true insolation, we can only explain by supposing that external conditions—notably temperature—may retard or counteract the preservative quality of the solar rays. This point is one which calls for careful investigation. It must be understood, however, that the putrefactive tendency of warmth does not in our experience, with this solution at least, override what we have termed the preservative quality of light; for, provided that there was a full amount of sunlight, we have preserved tubes exposed *continuously* from day to day as readily in hot weather as in cool.

An important result, we venture to think, in connexion with series *a* of Observation 8, is the appearance of mycelial fungus in the two tubes which were longest insolated.

In the course of our investigation we have found that, when *Bacteria* appear early and in large numbers in the solutions we have used, the mycelium of *Penicillium* or other microscopic fungus is rarely seen, the *Bacteria* apparently preoccupying the ground; when, however, the development of the *Bacteria* is from some cause retarded or prevented, we have frequently found tufts of delicate mycelium submerged in our experimental solutions, after they have been encased or removed into diffused light. An example of this is seen in Observation 3, and other instances might be cited. It is to be observed, however, that no mycelium appeared during the period of exposure of a solution, except under the conditions hereafter stated, nor, indeed, afterwards, if this were sufficiently prolonged.

We infer accordingly that light may retard or altogether prevent the appearance of mycelial fungi, but that its influence in this respect is slower and less powerful than upon the *Schizomycetes.*

May not this explain, in part at least, the sparing distribution of *Bacteria* in ordinary air as compared with the prevalence of the spores of *Penicillium* &c., a fact observed by Burdon Sanderson and others, and which our own experience tends to corroborate?

In the course of our investigation we found that, within certain limits, the rapidity with which *Bacteria* appeared in the solution A was proportionate to its *dilution.* This is illustrated in the following observation, in which we took advantage of the greater resistance to decomposition of the stronger solutions for the purpose of ascertaining whether diffused light exerts any appreciable influence on the processes under consideration. We had already observed that diffused light did not prevent the appearance of *Bacteria* in a solution when made in the strength given in the Appendix (*A*).

Obs. 9. *July* 24.—Four solutions are prepared, so that—

I. is of the ordinary strength.
II. is twice as strong.
III. is 3½ times as strong.
IV. is 5 „ „ „

One tube of each solution respectively is placed—

(1) In the dark;
(2) In the diffused light of a somewhat badly lighted room;
(3) In broad diffused daylight.

The result is seen in the following Table:—

(1) In the dark:

I. became turbid with *Bacteria*, July 27
II. „ „ „ „ „ 28
III. „ „ „ „ „ 29
IV. „ „ „ „ Aug. 2

(2) In a dull light:
I. became turbid with *Bacteria*, July 26
II. „ „ „ „ „ 28
III. „ „ „ „ „ 29
IV. contained a tuft of mycelium, Aug. 2, and became turbid with *Bacteria* and mycelium, &c., Aug. 5.

(3) In diffused daylight:
I. contained *Bacteria* .... July 29
II. }
III. } remained clear.
IV. }

It is right to state that the tubes of series 3 were inadvertently exposed to about twenty minutes of sunlight on July 24th; but we do not think that this materially interfered with the result, which demonstrates the preservative influence of diffused daylight alone, although in less degree than that of the direct solar rays.

The greater tendency of the more dilute solution to decomposition has been pointed out. We have again and again endeavoured to sterilize a solution one tenth of the strength given in the Appendix (*A*), but without success. Whether this failure was due to the unfavourable state of the weather and clouded skies which have invariably supervened, or whether in a solution of this strength the development of the *Bacteria* proceeds with such rapidity that a warm night may in its favouring tendencies outbalance the retarding influence of the day, we cannot say. We have, however, notwithstanding one or two failures in dull, close weather, repeatedly succeeded in sterilizing *urine*, and have at the present time in our possession tubes containing that liquid which has been preserved perfectly fresh and clear through the summer months.

One example is given.

Obs. 10. *July* 26.—Three test-tubes were partially filled with fresh urine of a golden sherry tint, and the mouth of each was guarded by a pledget of cotton wool. Two were insolated, one encased.

Aug. 1.—The contents of the encased tube were turbid and putrid, but the urine in the tubes which were exposed to the light remained perfectly pellucid. One of these (*a*) was now encased, the other (*b*) was left as before.

In about a week two small tufts of mycelium had appeared at the bottom of the tube marked *a*, the solution in which, however, was otherwise perfectly clear.

Oct. 13.—The urine in tube *b* was as clear as when the experiment was first started, nor could any thing except mycelium with sporidia be discovered in tube *a* on close examination with an immersion $\frac{1}{12}''$. The urine in this tube had a strongly acid reaction. On the other hand the urine in the tube which was encased from the first was so offensive as to render the examination of even a drop a disagreeable task. It contained rods and dumb-bells in great numbers, and an abundance of the micrococci associated with the ammoniacal fermentation of urea. The reaction was alkaline. Prolonged insolation, it may be noted, had a bleaching effect on the urinary colouring-matter.

Most of our preliminary observations have been made with Pasteur's solution and with urine, but more recently we have experimented on some hay-infusion.

Obs. 11. *Sept.* 22.—Three capillary tubes were filled with infusion made from some very old hay. One end of each was sealed off, the other end having a small plug of cotton wool. The infusion was of a deep yellow-brown colour. Two of the tubes (*a*) were insolated, and one (*b*) encased.

A portion also of the infusion was boiled for five minutes in a test-tube, the mouth of which was closed during ebullition with cotton wool, the tube (which was labelled *c*) being then placed in the dark.

Oct. 7.—Each sample was closely examined under the microscope. In the encased tube *b* large numbers of moving rod-like *Bacteria* were seen; but in the insolated tubes *a* a very few moving particles alone were visible to an immersion $\frac{1}{12}''$. The solution *c*, which had been boiled, contained a large number of rods, of greenish tint, with slightly clubbed refractive ends, for the most part motionless, and usually single. None of these were observed in the capillary tubes.

In some observations with turnip-infusion made in May last we found that while it became extremely rotten and offensive in those tubes which were encased, in corresponding tubes exposed to the light it was comparatively odourless, although the development of the *Bacteria* had not been wholly prevented.

Judging from the following experiment with *zymase*, light would not appear to exercise any retarding influence on the "indirect ferments."

Obs. 12.—Some yeast water, four or five times filtered, was mixed with weak syrup (which previously to the experiment was proved to have scarcely any action on Fehling's solution) and placed in two test-tubes. One was encased in the usual way, the other was exposed for two hours to full daylight, including about three quarters of an hour of direct sunlight.

At the end of this time,

25 grain measures of Fehling's solution were reduced by 110 grain measures of the insolated liquid.

25 grain measures of Fehling's solution were reduced by 112 of the solution from the encased tube.

These results may be regarded as being, within the limit of experimental error, practically the same.

What is the true account of this influence of light which has been shown to act so destructively on organisms by no means deficient in their tenacity of life? We do not profess to enter into this question at present any further than to state the results of one or two of the numerous experiments which we have made in this direction. The first question which presents itself is, with what rays of the spectrum is this property of light coincident? Is it localized in any one part, or is the unbroken pencil of rays necessary? The most definite result which we have hitherto obtained is embodied in the following experiment.

Obs. 13.—On Oct. 8th eighteen small test-tubes were partially filled with a solution, which was purposely made of twice the strength given in the Appendix, and were then plugged with cotton wool.

Three of these tubes were placed in each of four boxes, the sides of which were made of blood-red, yellow, deep blue, and ordinary glass respectively. Of the remaining six tubes, three were encased, and three were simply exposed to the light. All were placed as usual on an outside window-ledge facing south-east.

Oct. 11.—The encased tubes became turbid with innumerable *Bacteria.*

Oct. 12.—The tubes in the yellow box were all clouded with *Bacteria.*

Oct. 13.—The tubes in the red box showed signs of commencing turbidity, all the other tubes remaining up to this time quite pellucid.

Oct. 14.—All the red tubes were very turbid; two of the tubes in the box of ordinary colourless glass were also slightly turbid.

Oct. 17.—One blue tube has become slightly clouded with *Bacteria.*

The tubes as they broke down were all carefully examined with a $\frac{1}{12}''$ immersion. No organisms other than the ordinary rod-shaped *Bacteria* were seen in any, nor did these differ as regards their apparent vitality and activity.

The remaining tubes in the blue glass case, those simply exposed to the light (*i. e.* not placed in any glass case), and the surviving tube of the three exposed in the box of ordinary window-glass have remained perfectly clear to the date of writing (Oct. 17th). This experiment points forcibly to the actinic rays of the spectrum as the active agents; we hesitate, however, to affirm this positively for the present.

In the course of our investigation we have repeatedly had occasion to notice the proneness of mycelial fungi to appear (to the exclusion of *Bacteria*) in solutions which were themselves of a yellow colour, or were subjected to yellow light. Thus tubes containing urine exposed to light have several times developed a tuft of mycelium either during insolation or afterwards when encased.

The following observation illustrates the same fact as regards the artificial solution with which most of our experiments have been made.

Obs. 14. *May* 30.—Three small test-tubes containing solution A were suspended in three larger tubes containing solution of picric acid of such strength as to represent three gradations of colour from a barely perceptible yellow to a deep tinge of the same. Three corresponding arrangements were made with pure distilled water in place of the picric acid solution. A tube containing some of the same Pasteur's solution was encased in the usual manner and all were plugged with cotton wool.

June 5.—The encased tube became turbid.

June 8.—A tuft of mycelium appeared in the *medium* yellow.

June 9.—The *deep* yellow became clouded with *Bacteria.*

June 11.—Tufts of mycelium appeared in the *faintly* yellow.

The tubes suspended in distilled water became sterilized, nor could any thing save mycelial growths be discovered in either of the solutions exposed to yellow light, with the exception of that suspended in the yellow of deepest tint, in which *Bacteria* had appeared at an early period and excluded the mycelium. This result we attribute to the apparent fact that the less deep shades of yellow allow rays to pass which may at least check the development of *Bacteria*, but are less potent in their influence on the germs of the higher fungi, which accordingly develop the more readily since they have not to struggle with the former for the mastery. By this indirect influence, therefore, rather than by any special and direct action, we explain the tendency of mycelium to spring up in yellow light. This explanation, moreover, is in accordance with our deduction that, although the mycelial fungi are injuriously affected by light, they are nevertheless more resistant to its influence than *Bacteria.*

The breaking down in Observation 13 of the tubes in the case of window-glass is remarkable when contrasted with the survival of the solutions in the blue case. We have, however, invariably found it a difficult matter to sterilize an ordinary cultivation-liquid when a second screen of glass was placed between it and the light.

Early in the investigation it occurred to us that oxygen might be found to play a part in the phenomena under observation. Tubes containing solution A were therefore exhausted at the Sprengel pump and sealed off, our intention being, in the first place, to observe the effect of insolation in the absence of an atmosphere, filtered air being afterwards admitted. To our surprise we found in a preliminary experiment that on breaking the sealed points of the tubes under cotton wool, after the vacuum had been maintained for two days, the solutions were perfectly sterilized and could be preserved indefinitely. This fact (which, we have since been informed, was stated by Professor Tyndall in the Transactions of the Royal Society this year) cut off for the present this mode of approaching the problem.

The deductions which we draw from these simple experiments may be summed up as follows:—

1. Light is inimical to the development of *Bacteria* and the microscopic fungi associated with putrefaction and decay, its action on the latter organisms being apparently less rapid than upon the former.

2. Under favourable conditions it wholly prevents that development, but under less favourable it may only retard.

3. The preservative quality of light, as might be expected, is most

powerful in the direct solar ray, but can be demonstrated to exist in ordinary diffused daylight.

4. So far as our investigation has gone it would appear that it is chiefly, but perhaps not entirely, associated with the actinic rays of the spectrum.

5. The fitness of a cultivation-liquid to act as a nidus is not impaired by insolation.

6. The germs originally present in such a liquid may be wholly destroyed, and a putrescible fluid perfectly preserved by the unaided action of light.

Although there are many vital phenomena, both of plant-life and of animal, whether in health or disease, to the elucidation of which may be applied this quality of light (now demonstrated, so far as we are aware, for the first time), we have endeavoured in this paper to confine ourselves to the plain facts of our observations, and have studiously avoided speculation and theory. We cannot, however, refrain from offering one comment on the striking antagonism between these facts and many views that have hitherto prevailed on the relation of light to life. This relation has been principally investigated as regards the chlorophyl-cell; but chlorophyl may be regarded as simply an organ of nutrition adapted to special circumstances, and differing essentially in its vital phenomena from the true cellular tissue of the plant and its protoplasmic contents.

It appears to us that the organisms which have been the subject of our research may be regarded simply as individual "cells" or minute protoplasmic masses specially fitted by their transparency and tenuity for the demonstration of physical and other influences. May we not expect that laws similar to those which here manifest themselves may be in operation throughout the vegetable, and perhaps also the animal kingdom wherever light has direct access to protoplasm? On the one hand we have chlorophyl, owing its very existence to light, and whose functions are deoxidizing; on the other the white protoplasm, or germinal matter, oxidizing in its relations, and to which, in some of its forms at least, the solar rays are not only non-essential, but even devitalizing and injurious.

This suggestion we advance provisionally and with diffidence; nor do we wish to imply that the relations of light to protoplasmic matter are by any means so simple as might be inferred from the above broad statement.

APPENDIX.

The artificial solutions employed were similar to those used by Pasteur. The following are the formulæ employed:—

A.—Water, 1500.
Brown Sugar Candy, 70.
Tartaric Acid, 4.
Ammonium Nitrate, 4.

Potassic Carbonate, 0·6.
Ammonium Phosphate, 1.
Solution neutralized with Ammonia and filtered.

B.—Approximately the same as last, but the ingredients not weighed.

Unless otherwise stated, the tubes were always prepared and exposed as in Observation 1.

It is self-evident that tubes exposed with a south-easterly aspect would receive but a fraction of the total solar rays each day. We have not, however, been able to place them in a position where they would be under the direct influence of the sun during the whole period that it was above the horizon; with such an arrangement we should expect to obtain results proportionately greater.

[Received November 5, 1877.]

POSTSCRIPT.

We have stated in the preceding paper that, on exhausting tubes containing solution A by means of a Sprengel pump and sealing them, we found that not only, as might be expected, did no development of organisms occur under these conditions, but that if the vacuum was maintained for a sufficient length of time, the solution became absolutely *barren.* Knowing the necessity of oxygen to *Bacteria* (of the ordinary kind at least), and taking into consideration the products of their "respiration," we inferred that this result was attributable to the absence of oxygen, and consequent asphyxia not only of the mature forms of those organisms visible to the higher powers of the microscope, but also, it necessarily followed, of that rudimentary "germinal" material which, eluding even the piercing test of the electric beam, is distributed with extraordinary uniformity in almost every water.

We observed, also, that we have since learned that this mode of sterilization has been recently demonstrated by Professor Tyndall, with a similar interpretation, and on this account we did not consider it worth while to enter upon any details of our own experiments in this direction. Within the last few weeks, however, we have, by employing *urine* as the experimental fluid, obtained results of considerable interest, and have thought it well to append some account of them.

Observations with tubes exhausted at the Sprengel pump.

Obs. 1. *July* 18.—Two tubes containing Pasteur solution were exhausted. The one (*a*) was at once sealed off; the other (*b*) was left attached to the pump (the vacuum being maintained) for three hours, air carefully filtered through cotton wool being then gradually admitted. On July 23 the contents of *b* were found to be turbid.

On July 26 air was admitted with similar precautions into tube *a* by breaking the sealed capillary end within a ball of cotton wool. This tube is still perfectly pellucid [Nov. 3].

We concluded from this experiment that while three hours' vacuum was insufficient to sterilize the solution employed, one of eight days duration rendered it absolutely barren, the latter fact being confirmed by several repetitions of the experiment, one of which demonstrated the sterilization of the solution by two days' vacuum.

On substituting *urine* for Pasteur solution different results were obtained.

Obs. 2.—A tube containing fresh acid urine was on September 19 exhausted and sealed off. On September 22 the capillary point was broken under cotton wool. On September 29 the contents were found to be turbid with *Bacteria.*

Having thus ascertained that urine resisted the sterilizing effects of a vacuum for a period sufficiently long to enable us to test the effects of insolation on this fluid *in vacuo*, we proceeded to carry out our original plan of investigating the more intimate nature of the processes by which light exerts its germicidal action. The result of this inquiry, so far as it has gone, is shown in the following observation:—

Obs. 3. *Oct.* 26.—Of eight test-tubes containing fresh acid urine, two, which we will call *aa*, were simply plugged with cotton wool and insolated in the ordinary way; two, labelled *a'a'*, were plugged and encased. The four remaining tubes were exhausted at the Sprengel pump and sealed; two, *bb*, were exposed for insolation, the remaining two, *b'b'*, being encased.

Oct. 30.—*a'a'* were both swarming with *Bacteria.*

Oct. 31.—The exhausted tubes, *both insolated and encased* (*bb* and *b'b'*) were distinctly and nearly equally turbid, the degree of cloudiness being, if any thing, more marked in the insolated tubes *bb.*

The urine in the tubes *aa*, which were insolated in the ordinary manner, contained numerous small points of submerged growing mycelium, but with this exception was perfectly bright and clear.

Nov. 2.—On examination with a $\frac{1}{12}''$ immersion objective numerous rods in active movement were seen in each of the four tubes which had been exhausted.

The tubes *a'a'* also swarmed with bacterial life; their contents were putrid and slightly alkaline in reaction. The contents of the exhausted tubes had in each case a disagreeable putrefactive odour, but the reaction was still acid.

The urine in tubes *aa* was acid and fresh in odour. With the exception of numerous mycelial tufts and one or two moving rods in the meshes, nothing was seen in these tubes on microscopical examination. This experiment was a repetition of two previous observations which gave similar results and need not be detailed.

In all cases exhaustion at the Sprengel pump was carried on until ebullition occurred in the liquid operated upon, and the mercury had for a minute or two fallen with a well-marked "water-hammer" click. No gauge was attached to the pump. We do not, of course, regard such a vacuum as perfect; but it was sufficient for our purpose, and, as regards the Pasteur solution, proved fatal to the contained organisms.

In the experiment of which *Obs.* 3 is here given as an example we observe, on the one hand, the prevention of bacterial development and consequent growth of mycelial forms (the quantity of light being insufficient for the destruction of these) in those tubes which were insolated in the presence of ordinary atmospheric air. On the other hand we see specimens of the same urine insolated to precisely the same degree as the former, but, *in the absence of an atmosphere*, becoming turbid, even *in vacuo*, with *Bacteria* as early as their encased congeners.

This remarkable fact, then, appears to follow as a deduction, that a vacuum (or approximation to such) which of itself is a condition antagonistic to the development of *Bacteria*, nevertheless shields these organisms from the germicidal effect of light[*].

It is not our present purpose to speculate on the interpretation of the phenomena here presented, nor should we be justified in so doing until we have further extended our observations, and more fully confirmed the curious results here provisionally detailed.

IV. "Points of Resemblance between the Suprarenal Bodies of the Horse and Dog, and certain occasional Structures in the Ovary." By CHARLES CREIGHTON, M.B., Demonstrator of Anatomy, Cambridge University. Communicated by Professor HUMPHRY, F.R.S. Received October 12, 1877.

(Abstract.)

The object of this communication is to prove, with the aid of accurate drawings, that there exists an essential resemblance between the constituent parts of the suprarenal bodies of mammals and certain structures in the mammalian ovary that are of occasional but normal occurrence. The appearances on which the comparison is based are best seen in the suprarenals of the dog and horse, and in the ovaries of the bitch. The suprarenals of the horse and dog are known to have, immediately under the fibrous tunic, a zone of follicles of singular though well-defined structure. The first point in the communication is one of criticism, and has reference to the division of parts within the suprarenal. It is held that the outer zone of follicles, as they are seen in the horse and dog, are quite unique among the structures composing the suprarenal, and are broadly contrasted with the rest of the organ lying internal to them. The contrast is unmistakable in these two animals, and it is equally

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[*] We wish, however, to make it clear that we by no means insist on this explanation; the facts, indeed, admit of other explanations.

well-marked in the fœtal state of the organ in other mammals. In opposition, therefore, to the usual subdivision of parts, it is proposed to limit the term "cortex" to the extreme outer zone consisting of the peculiar structures above-mentioned, and to apply the term "medulla" to the general mass or parenchyma of the organ which the outer zone covers, including in the medulla the extreme central and generally pigmented part to which the term "medulles" has been hitherto limited. This proposed rearrangement of terms is based upon the fact that the outer zone has no continuity of structure with the zones next to it; whereas the extreme central part differs from the neighbouring parenchyma in unessential particulars, and chiefly in the character of the blood-vessels or blood-spaces within it.

The comparison with structures in the ovary is based upon the above radical distinction of cortex and general parenchyma in the suprarenals, and is worked out in two sections dealing with the two parts respectively. The structures in the ovary to which the cortical follicles are compared are here described for the first time, and the discovery of them, and of their nature, has been the starting-point of the present investigation. They are the remains of Graafian follicles within which the ovum, after reaching a degree of ripeness, has shrivelled up and disappeared. The appearances in question are numerous in the ovaries of the bitch, especially towards old age. Their structure is perfectly definite and their occurrence tolerably uniform. They are spoken of as obsolete Graafian follicles, and are to be carefully distinguished from those Graafian follicles from which a ripe ovum has been successfully discharged.

The ovarian structures to which the general parenchymatous mass of the suprarenal is compared are persistent *corpora lutea*, entirely solid and cellular. This part of the comparison occupies the second section of the paper, and the details of it are referred to at the end of the abstract.

Coming to the details of his comparison, the author first states the facts relating to the cortical suprarenal structures. Their form, especially in the horse, appears different in the surface section and in the perpendicular section. But, premising that the same differences of shape occur among the corresponding appearances in the ovary, they may be taken to be essentially elongated closed cylinders, straight, or curved, or tortuous, or doubled up. The cylinders are completely filled with long and narrow epithelial-like cells arranged in close order across their lumen, *i. e.* at right angles to their long axis. Each cell stretches across, generally speaking, the whole width of the cylindrical space; the nucleus of a cell is generally towards one end, and the other end is often pointed. Cells appear alternately to arise from opposite sides of the space, their pointed or relatively free extremities interlocking among the nuclear or basal ends of the cells opposite.

The same appearances are produced in the ovary in the following way. The Graafian follicle may be in a more or less advanced stage of ripeness. Either the *membrana granulosa* may closely invest the ovum, or it may be separated from the ovum (within its *cumulus proligerus*) by the cavity for the *liquor folliculi*. In the former class of cases, the *membrana granulosa* has a short circuit, in the latter it has a very much longer circumference. Within a follicle of either class the ovum decays, and the remains of it are expelled or absorbed. The circuit of the *membrana granulosa* has been broken, and the circular belt of cells is found either to have straightened itself out, if it were of small extent, or to have become folded or thrown into sinuosities if it belonged to one of the more distended or riper class of follicles. What remains of the aborted Graafian follicle is the *membrana granulosa*, in the form of a longer or shorter cylindrical body, straight, or slightly curved, or sinuous, or doubled up. These long, narrow, and variously curved cylinders, as they are found, in their ultimate state of quiescence, in the substance of the ovary, have the most remarkable superficial resemblance to the cortical cylinders of the suprarenal. But the resemblance in minute structure is still more remarkable. While the ovum was shrivelling up or decaying, the round and nuclear cells of the *membrana granulosa* were taking the shape of long and narrow columnar epithelial cells, stretching from one side of the belt to the other. These cells have their nucleus towards one end, and the cell is seated by that end either upon the outer or concave circumference of the belt, or upon the opposite convex or inner circumference, the two sets of cells interlocking with their free ends across the space. Comparing a given length of such ovarian cylinder with a corresponding length of cylinder in the suprarenal, the minute points of resemblance are as follows:—Both cylinders are completely filled with cells packed in close order across their long axis; the cells have precisely the same length and breadth, the same relation of attached ends and free ends, the former uniformly broad and containing the nucleus, and the latter pointed and interlocking with the attached end opposite. As regards cell-substance, and size and shape of nucleus, no differences are discernible.

The structures from two different organs, that are here brought into comparison, not only resemble one another closely, but they are each of them unlike any thing else in the body. The origin of the structures in the ovary can be traced in the clearest manner; they are the remains of Graafian follicles within which the ovum has aborted or decayed. The conclusion is that the cortical structures of the suprarenal are the obsolete condition of follicles that, in their active period, resembled the existing Graafian follicles; and that conclusion is so far in accordance with the hypothesis, based upon independent evidence, that the suprarenal body as a whole (and in its several parts) is an obsolete organ.

In the suprarenals of other mammals besides the dog and horse, the structure of the cortical zone is not of a kind to suggest or to bear out the above comparison. But, according to Henle, the peculiar structures of the horse and dog are occasionally found in other mammals. An occasional reappearance is what might be looked for in an obsolete structure. But, exact similarity apart, there is in the suprarenals of all mammals a *zona glomerulosa*, which corresponds to the cortical zone above described; and the *zona glomerulosa*, being subject only to laws of heredity, and not to laws of function, may be considered in most mammals to have lost those distinctive features which survive, for unexplained reasons, in the dog and horse. Again, as regards the parallel structures in the ovary, they are seen clearly in the bitch, are indistinctly seen in the cat, and are not to be distinguished in the mare. But those differences are not fatal to the generalization, and explanations are offered of them.

In the second section of the paper the author describes the resemblance of the general intracortical mass of the suprarenal to another class of ovarian structures, viz. *corpora lutea*. The *corpora lutea* here spoken of are large cellular masses of very definite and stable structure, occurring at various points in the ovary, and in some cases of so great extent as to transform the ovary into an organ of altogether new appearance. Instances are referred to of ovaries from the mare and bitch in which *corpora lutea* seemed to have greater persistence than is usually attributed to them, and it is held that the prevalent theory of the circumstances of their formation and decay is not comprehensive enough.

Taking, however, the *corpora lutea* as they are found, and directing attention to their minute structure, they have the following points of resemblance to the intracortical mass of the suprarenal. Both structures are cellular throughout, and the individual cells are the same. The cells are epithelial-like and polyhedric, with a central nucleus and a wide zone of cell-substance. The cell-substance in both cases is so coarsely granular as often to resemble the vitellus of the mammalian ovum, and in both cases the granular protoplasm is sometimes replaced or occupied by a vacuole. The one point of difference is that the cells of the *corpus luteum* are half as large again as those of the suprarenal parenchyma. The second point is that the cells in both cases are set in a fine meshwork of fibres connected with the walls of the capillaries. Thirdly, there is, in both, the same radial arrangement of blood-vessels (capillaries) from centre to circumference. Fourthly, the central vein or system of venous lacunæ of the suprarenal has its counterpart in the *corpus luteum*. Lastly, there are points of resemblance relating to the pigmentation of the respective structures.

Although it is not essential to the justice of the comparison that the obsolete Graafian follicles should have the same position round the circumference of *corpora lutea* which the cortical suprarenal follicles have round the parenchyma of that organ, yet there is to be seen in many preparations a curious similarity in that respect also.

The present communication does not go beyond a statement of points of resemblance. The *corpus luteum*, which enters so largely into the comparison, is itself as much an unsolved problem as the suprarenal. It may be said, however, to afford better opportunities of study; and if the resemblance above outlined be a resemblance in essentials, a sound theory of the suprarenal as a whole will probably be found to depend upon a sound theory of the *corpus luteum.*

December 13, 1877.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Among the Presents was a transparent positive photograph of the sun on glass, taken at Meudon by M. Janssen, For. Mem. R.S., and presented by him to the Society.

Pursuant to notice given at the last Meeting, Marcellin Berthelot, Joseph Decaisne, Emil Du Bois-Reymond, Adolph Wilhelm Hermann Kolbe, Rudolph Leuckart, Simon Newcomb, and Pafnutij Tschebytschew were balloted for and elected Foreign Members of the Society.

The following Papers were read:—

I. "On Electrostriction." By EDMUND J. MILLS, D.Sc., F.R.S., "Young" Professor of Technical Chemistry in Anderson's College, Glasgow. Received August 7, 1877.

If the bulb of an ordinary thermometer be coated chemically with silver, and then electrically with a metallic deposit, the mercury will traverse some portion of the scale, and finally take up a definite position, independently of temperature. To this phenomenon I have given the name *electrostriction.* Of the metals hitherto worked with, copper, silver, iron, and nickel constrict the bulb; zinc and cadmium distend it.

The general conditions under which the experiments were made were as follow:—A thermometer coated with silver by immersion in a solution of ammoniacal argentic tartrate was placed vertically near a bare thermometer at one side of a depositing cell; the anode stood at a distance of 11 centimetres. The bulbs of the thermometers were about their own depth below the surface of the electrolyte; the covered one was turned half round at every comparison. The source of electricity was a pint

Daniell's cell, having a porous diaphragm, and the circuit included a galvanoscope. Observations were made at definite intervals of time, immediately after stirring the liquid; and the difference between the two scales, after suitable reduction, was registered as electrostrictive effect. The temperature was in all cases the unrestricted temperature of the laboratory.

Copper.—The thermometer ("454") first used had a cylindrical bulb, which was slightly concave towards its vertical axis. The electrolyte consisted of the ordinary acidified solution of cupric sulphate, containing zincic sulphate.

In the following Table the column below x contains the time in hours; that below y the number of scale-units ("degrees" C.) remaining to be traversed. The latter are compared with a series of values calculated from the equation—

$$y = 9{\cdot}859\,({\cdot}96363)^x + 1{\cdot}358\,({\cdot}71841)^x.$$

TABLE I.

x.	y.	y calc.	Temp.
1	10·520	10·476	12°·2
2	9·917	9·856	12·9
3	9·316	9·325	13·9
4	8·822	8·863	14·7
5	8·467	8·452	15·5
6·5	7·825	7·907	16·1
8	7·359	7·427	16·7
9	7·276	7·133	17·4
10	6·979	6·856	18·1
48	2·302	1·665	12·2
		Mean temperature	15·0

The difference between experiment and calculation in the last value of y is doubtless due to the considerable amount of stratification observed in the electrolyte, which had not been stirred subsequently to the last observation. Omitting this result, the probable error of a single comparison of the calculated with the found values of y is ·056. The calculated total ascent is 11·22 scale-units. By continuing the preceding experiment, but with two Daniell's cells, for 23½ hours longer, 10·933 units were actually attained.

A thermometer ("502") having a very nearly spherical bulb was next employed; its polar axis (a line representing the prolongation of the mercurial thread) measured 11·73 millimetres, its equatorial axis 11·55 millimetres. This instrument was prepared in the same manner as the preceding, immersed vertically in the electrolyte, and turned halfway round every half-hour. Even deposition on a sphere is, under such circumstances, out of the question, most of the metal being precipitated on its equatorial, and much less on the polar regions. The registered rise was the height of the mercury in melting ice. After the twenty-second hour, and as soon as the scale had been read with the kathetometer, the mercury rose spontaneously 0·16 of a unit. The subsequent observations are accordingly lessened by this amount. The equation is—

$$y = 9{\cdot}728\,({\cdot}87074)^x - 4{\cdot}979\,({\cdot}83117)^x.$$

TABLE II.

x.	*y*.	*y* calc.	Temp.
1	4·613	4·332	16°·2
2	3·996	3·936	16·6
3	3·556	3·563	16·8
4	3·270	3·216	15·7
5	2·941	2·894	16·5
7	2·321	2·328	15·5
9	1·815	1·856	14·7
11	1·462	1·471	12·7
13	1·242	1·159	15·7
15	0·973	0·909	16·3
17	0·708	0·710	17·2
19	0·553	0·553	17·2
21	0·475	0·430	15·2
23	0·335	0·332	16·5
25	0·251	0·257	14·5
		Mean temperature	15·8

The above values of *x* are convertible into hours by multiplying by 2.

The probable error of a single comparison of theory with experiment is ·057. After the fiftieth hour the operation was left unattended for 28 hours, when an additional rise of ·25 was found to have taken place.

On other occasions an ascent of 12·87 was obtained, with thermometer 454, in twenty-four hours, the thickness of the deposit being approximately ·29 millimetre. An ascent of 11·50 was also observed with thermometer 455, the thickness of metal being probably not less than a millimetre. The zero of thermometers was found to be nearly the same after electrostriction and stripping as at first; it had not risen more than 0°·03.

Silver.—Thermometer 454, slightly coated with silver by electro-deposition, was used in the following experiments: the coating was first sand-papered and polished. Observations were made and the thermometer semirotated every half-hour until the fifth hour; the electrolyte was not stirred between the fifth and twenty-third hours, but after this interval the ordinary course of observations was resumed. The equation is—

$$y = 10{\cdot}826\,({\cdot}96179)^x + 1{\cdot}535\,({\cdot}82044)^x.$$

TABLE III.

x.	*y*.	*y* calc.	Temp.
1	11·670	11·671	19°·1
2	11·010	11·047	19·7
3	10·517	10·479	20·2
4	10·062	9·959	20·6
5	9·400	9·480	20·7
23	4·358	4·435	15·1
24	4·250	4·263	15·7
25*	4·193	4·099	16·3

Mean temperature 18·4

The probable error of a single comparison is ·047.

Iron.—The bulb of thermometer 455 was prepared as usual, and then coated during two hours with copper. Electro-deposition of iron was next proceeded with, and the following results recorded. Between the third and fourth hours a slit was seen to form at the bottom of the bulb, and gradually extend to the shoulder.

TABLE IV.

Hours.	Total ascent.	Temp.
1	0·883	17°·5
2	1·775	17·3
3	2·193	17·4
4	1·648	17·6

It must be extremely difficult to obtain a coating of iron of any considerable thickness.

Nickel.—Thermometer 454 was used. The anode and kathode were 95 millimetres apart. The electrolyte consisted of a neutral solution of ammonio-nickelous sulphate; the anode of nickelous carbide.

TABLE V.

Hours.	Total ascent.	Temp.
2	0·096	18°·6
3	0·474	19·2
4	0·759	19·8
5	0·856	20·6
6	2·314	19·8
24		

When last observed the deposit had split outwards, beginning at the side opposite the anode.

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* Mean of 24·5 and 25·5.

Zinc.—Two pint Daniell's cells were employed, and thermometer 454. The results were as follows :—

Table VI.

Hours.	Total descent.	Temp.
0·5	0·352	12°·4
1·0	0·308	12·5
1·5	0·366	12·7
2·5	0·404	13·2
3·5	0·382	13·8
4·5	0·574	14·5
47·0	0·749	11·6

It is probable, from the graphic projection of these numbers, that the descent attained after the forty-seventh hour would have been little, if at all, exceeded by prolonging the experiment.

Cadmium.—The conditions were those of the preceding set of observations.

Table VII.

Hours.	Total descent.	Temp.
1·0	0·185	15°·0
2·0	0·274	15·5
3·0	0·164	15·8
5·0	0·166	16·8
23·5	0·207	15·3

At the end of the experiment the deposit was found to be somewhat corrugated, but fairly satisfactory. The electrolyte had the disadvantage of being freshly prepared, and not having been continuously worked prior to this deposition.

Value of the Electrostrictive Effect.

In order to ascertain the value of the electrostrictive effect, thermometer 454 was adjusted in a water-pressure apparatus capable of indicating about 2¼ atmospheres: it was then submitted to uniform compression, and the resulting ascent of the mercury in its stem noted. Two sets of observations at higher pressures showed, after the usual reductions, that a unit of scale corresponded to 8·2 and 8·5 atmospheres respectively; the mean is 8·3 atmospheres. After this the bulb of the instrument was unfortunately broken, while preparing it for considerably increased compression. I have, however, little hesitation in accepting this result as a datum for calculating the total electrostrictive effect at much higher readings. Similarly with thermometer 502, a depression of 0°·199 C. was caused by opening; this would correspond to 5·03 atmospheres per scale-unit. Again, ten experiments with thermometer 455 were conducted in an Andrews's duplex apparatus (a most admirable instrument for such a purpose), at pressures varying from 49·4 to 133·6 atmospheres. By these it was found that a scale-unit corresponds, in this case, to 9·44 atmospheres, with a probable error of 0·1 atmosphere for a single determination.

The above data have led to the construction of the following table.

TABLE VIII.

			Extreme effect	
Metal.	Therm.	Effect.	in scale-units.	in atmospheres.
Cadmium	454	Distension	0·274	2·3
Zinc	„	„	0·749	6·2
Nickel	„	Compression	2·314	19·2
Iron	„	„	2·193	18·2
Silver	„	„	8·006	66·4
Copper	„	„	10·933	90·7
„	„	„	12·87	106·8
„	502	„	14·707*	74·0 †
„	455	„	11·50	108·5

It thus appears that the extreme force at work on the bulbs must have been very considerable.

Subsidiary Experiments and Remarks.

(1) An instrument having a general resemblance to a thermometer was constructed with a cylindrical caoutchouc bulb, about 30 millimetres long by 11 millimetres wide. The bulb was rendered conductive, and then made a kathode in the copper bath already referred to; it was frequently turned half round. In the course of a day it had become coated with a sufficiently thick layer of metal, and had then undergone most palpable collapse. We have here, therefore, a real case of local constrictive effect.

(2) Two flat microscopic slides of the usual size were silvered chemically and then made the kathode in a similar circuit. After twenty-four hours, the deposited copper on both slides was found to be arched downwards and inwards, towards the anode; the deposits were in fact, owing to their contiguity, to a great extent united. The upper parts of the deposits had torn themselves away from the glass; and two close parallel lines cut through the silver of one of the plates had the copper between them curled inwards like a gutter. From this evidence it was inferred that the cylindrical deposit, if cut in the direction of its length, would spring open. Such was found to be actually the case even with the copper deposited on the caoutchouc bulb; and to a still greater extent with copper overlying some of the same caoutchouc, where that material was stretched over a glass shoulder for the purpose of fastening, and there did not admit of collapse. The opening backwards of nickel and iron films has already been noticed.

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* Calculated. † This number may be wrong in either direction by 1½ atmosphere.

(3) The electrostrictive effect brings vividly to the mind the ordinary phenomenon of contraction during cooling—differing, perhaps, but little from that phenomenon, except in the circumstance that it is brought about without necessary change of temperature. The electrolyte is, in a manner, a melted metal. This view did not, of course, admit of experimental illustration in the case of the metals to which reference has been made. It was, however, conceived that substances other than metallic should yield the result in question, and a specimen of hard paraffin was selected for trial. This was heated to a point not far from that of fusion, and thermometer 454 was dipped into it several successive times. The following table shows the constriction obtained.

No. of dip.	Total ascent.	Temp.
1	·256	49°
2	·444	30
3	·633	—
4	·447	30
5	·369	30
6	·321	30

These results were confirmed by a second set of experiments. In each set, the coating of paraffin was found to he vertically fissured before the conclusion of the observations; this accounts for the gradual diminution of the effect.

(4) Electrostricting metal appears to be in a partially unstable condition. Thus a decided fall is observable in a coated thermometer after the deposit has had its surface disturbed by sand-papering or filing; this was noticed both with silver and copper. On the other hand, the constrictive effect is increased by heating the deposit to 100°, or by keeping in ice. Thermometer C, for example, having had its zero raised electrostrictively to 3°·52, showed an elevation to 5°·00 after heating for ten minutes to 100°, afterwards falling permanently to 4°·86. Thermometer 50 had its zero raised in a similar manner to 1°·87; this was increased to 2°·96 by a temperature of 100°. In another experiment with the same thermometer, a like increase from 0°·65 to 0°·79 was recorded; the latter number rose spontaneously to 0°·90 in the course of six months. Again, two similar thermometers (103 and 104) were electrostricted at about 10° C., and the zero was found to rise by heating, as before. The zero of 103 then fell gradually, during nine months, from 4°·73 to 4°·37; that of 104 fell during two months from 2°·01 to 1°·80, and then rose during seven months to 2°·14. Thermometer 101, made exactly on the same pattern, but left uncoated, exhibited the usual continuous ascent of zero during the same period, viz. from 0°·07 to 0°·31 in nine months. *All three thermometers showed identical and normal readings at* 100°. Facts like these prove that the nature of the deposit requires further elucidation. In any case the best evidence of electrostrictive effect is gained,

with copper at least, at the melting-point of ice; and it is not improbable that copper deposited at or near 100° would not show any such effect at any temperature. One circumstance may be mentioned as bearing on this point, viz. that the electrostrictive effect is greater, and more rapidly produced, at the winter temperature than at that of summer. It may be added that soft metallic deposits are, as a rule, obtained from heated electrolytes.

(5) Uniform external pressure on a cylinder takes effect chiefly on the diameter, and less on the length; and these results are practically independent of each other *. If we conceive such pressure to be produced by the electric deposition of successive layers of metal (as was actually the case with thermometer 502), it seems reasonable to suppose that each of the layers will have nearly the same constrictive effect. But the first layer constricts the bulb alone; the second constricts the first also; the third its predecessors, and so on. Hence the observed effect upon the bulb should diminish at compound interest. If y be the total obtainable effect after a time x, D the portion of it due to diametral constriction, L the portion of it due to longitudinal constriction, and d and l the respective geometric factors, we have, in the case of the cylindrical thermometer,

$$y = \mathrm{D}d^x + \mathrm{L}l^x,$$

D being always greater than L. On the other hand, the spherical thermometer necessarily receives more metal upon its equatorial region than can be deposited above or below, that region being nearer to the anode; and the longitudinal constrictive action must tend to bulge the equatorial part, opposing the constriction at work there.

Hence we have, for the sphere,

$$y = \mathrm{D}d^x - \mathrm{L}l^x,$$

D being the greater as before. Nevertheless it appears probable that in both cases, at the early stage of deposition, the difference between diametral and longitudinal compression is but slight, not having yet been much multiplied by the increased stress. Hence, during that period, an equation

$$y = \mathrm{D}d$$

may doubtless more accurately represent the experimental results. Subject to this and some other minor considerations (the close discussion of which would be out of place in mere indicative work) there is a fair agreement between theory and experiment. The probable error, in fact, of a single observation does not on the whole exceed ·053 of a scale-unit, being about ·4 per cent. on the total quantity determined; and this reduces to less than ·3 per cent., if we exclude the first comparisons made at a lower temperature, in Table I.

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* Fairbairn, Phil. Trans. 1858, p. 404.

(6) We have seen in (4) that metals may be deposited on a thermometer bulb and then deprived of a determinate part of their electrostrictive power, and that there is an experimental possibility of depositing them with none of this effect. We are therefore in a position to compare, one with the other, the two states of the same metal. The electrostricted metal (submitted, as may have been the case, to more than a hundred atmospheres' compression) must oppose the effect of a solvent much more than that which is soft and no longer the subject of electrostriction. In other words, a gramme of the former should require more of a reagent to dissolve it than should the latter, under the same conditions. But this excess corresponds to a known amount of electrostrictive effect, which, again, is known in atmospheres pressure. We ought therefore to be in a position to measure chemical effect in atmospheres pressure.

My best thanks are due to Professor Sir William Thomson for the loan of an Andrews's apparatus; to Professor George Forbes, for a pressure machine; and to Mr. W. H. Walenn for much practical advice in connexion with electro-metallurgy.

II. "The Examination of Air." By R. Angus Smith, Ph.D., F.R.S. Received August 11, 1877.

It is now many years since I first began to examine air so as to obtain decidedly those bodies which have from the earliest times been supposed to exist in it, bringing with them, on certain occasions, some of the worst results. About eight years ago (that is, in 1869), after giving a short summary of some of my work in the Journal of the Scottish Meteorological Society, I used these words :—" For a satisfactory investigation of the subject, one must look to the multiplication of these experiments, and perhaps to the establishment of a department at some Observatories for Chemical Climatology and Meteorology."

Afterwards, in 1872, I published an octavo volume on Chemical Climatology, embodying many of my results, and intended to be a beginning of work to be continued by a public body. Indeed some of the work was done whilst I was acting under the Government and was published in Government Reports. It was often my intention to begin a movement which might result in a fuller recognition of the claims of Chemical Climatology; but I have not gone further than speaking of it to Mr. Scott, and proposing it to the Local Government Board, since the time when I published the proposal alluded to; at least I may say I have not gone further in a distinct public manner.

I should have been glad had my work caused in this country a beginning such as has been made lately at the observatory of Mont Souris, at Paris, or at least resembling it; and I blame myself for not pushing forward the idea, although my numerous engagements may well form a kind of apology. My present object is partly to draw attention to the fact of my proposal and not to let it be forgotten, and it is also combined with another object, namely, to speak of methods of study or experiments which have been tried.

In obtaining specimens of any soluble or insoluble body from the atmosphere, I have *washed* the air with pure water—that is, I have shaken the water in a vessel of the air to be examined.

The other method, chiefly, and I may almost say universally, used, except by me, has been to pass the air through water.

It is of great importance to obtain the characteristics of each method. It is now many years since I passed air through water for three months and obtained so little of a result, that I gave up the matter for some time in despair. I passed air which I knew to contain acid and salts through water; but I obtained scarcely any of either, and I afterwards found both in the tubes through which the air passed before entering the water. This little incident made an era in all my inquiries. I tried minutely pointed tubes, but did not succeed, and for the same reason, or rather for that and an additional reason, viz. the minuteness detained the air longer in the tubes, and the result was worse.

A considerable number of small holes pierced in a bulb at the end of a glass tube having been recommended, I obtained, by the kindness of Mr. Dixon, of Glasgow, a few which he had got made in a beautiful and ingenious manner. I knew from my previous results that the success would be imperfect; but I thought it well to try any new method and to overlook all my work, quite willing to find that I had been doing in a laborious and imperfect manner that which could have been done with ease and rapidity. I did not find that Nature had altered her habits since my previous and numerous trials; but I found that the small holes prevented still more the exit of particles which floated in numbers in the bulbs.

The tubes retain not merely visible particles but those invisible, although by accumulation some of the latter also become visible, and give a turbid character to the liquid or a dimness, according to the amount of moisture and the hygrometric character of the substances.

The washing of the air is laborious, because the water must be shaken up violently within the bottle. In order to refill the bottle with air, an air-pump must be used until the required point is obtained on the mercury gauge, this being found to indicate a known amount of air, which is now allowed to enter so as to be washed exactly as the former quantity. To obtain the hydrochloric and sulphuric acid only in the air of Manchester, this operation must be repeated fifty times; with some pure air as much as 200 bottles have been used. It is not to be supposed that in such laborious measurements the fullest accuracy is attained; but, as I have said elsewhere, at least good comparative results are found. I know well the great importance of obtaining a ready method, and I am not surprised at others clinging to it, even when imperfect; but it is no less important that I should give my experience.

For ordinary cases I obtain sufficiently accurate results by apparently less exact measurements. With carbonic acid it may be possible to use aspiration with good results, because this acid does not adhere to the tubes; and by an increase of bottles for absorption all may be safe; but the increase of absorbing vessels is troublesome.

With salts and acids, such as hydrochloric and sulphuric, the difficulty of absorption is not so great; but the adherence to the tubes is remarkable, and there is no hope of any good result without washing out the tubes which give a passage to the air.

With organic substances both the difficulties occur, adherence to the tubes and passage through the solutions. I doubt if it is possible to wash out the tubes sufficiently; in such cases, the result is worse according to the length of the passage. I have tried various ways to overcome the difficulty, but have found only one way, and that is a very laborious one. These points have long been known to me, and for some time have been spoken of; but I add a few results so as to impress them on others. The importance of having a chemical department to our observatories cannot be long overlooked.

Bellows pump and shaking compared with aspiration and rose-ended tubes.

(Air at back of Devonshire Street, Manchester.)

Aspiration.

Volumes CO_2 per million volumes air.

EXPERIMENT 1.

1st bottle gave	89
2nd „	63
3rd „	58
Total	210 in series of 3 bottles.

EXPERIMENT 2.

1st bottle gave	24
2nd „	15
3rd „	13
Total	52 in series of 3 bottles.

EXPERIMENT 3.

1st bottle gave	52
2nd „	22
3rd „	35
Total	109 in series of 3 bottles.

EXPERIMENT 4.

1st bottle gave	41
2nd „	17
3rd „	53
4th „	35
Total	146 in series of 4 bottles.

409 vols. CO_2 per million vols. air by shaking.

These results are very irregular. It was thought that the solution of barium hydrate used was rather weak. With a much stronger solution the following experiments were made.

Aspiration.

Volumes CO_2 per million volumes air.

Experiment 5.

1st bottle gave	80
2nd „	62
3rd „	62
4th „	53
5th „	18
6th „	45
Total	320 in series of 6 bottles.

Experiment 6.

1st bottle gave	115
2nd „	71
3rd „	66
4th „	62
5th „	62
6th „	62
Total	438 in series of 6 bottles.

320 is low for Manchester; 438 is probably correct for the spot tried.

It is clear from these experiments that a series of three washing-bottles containing barium hydrate are not sufficient to absorb the carbonic acid from the air aspirated through them; with a series of six bottles it is troublesome to work, but more, and perhaps all, is obtained. Equal speeds are compared here and equal solutions. With the three absorbents the strong baryta solution gave still too little.

Sulphuric Acid (Sulphates) in the air.

Devonshire Street, Manchester, 1876.	By shaking.		Aspiration with 3 wash-bottles.	
	Grammes per million cub. metres.	Grains per million cub. feet.	Grammes per million cub. metres.	Grains per million cub. feet.
Yard: Nov. 10, clear afternoon ...	3482	1521	4612	2015
„ „ 11, fine, frosty morning	4352	1902	1731	756
„ „ 13, raining; sheltered from rain.........	4352	1902	3458	1511
„ „ 14, ditto, ditto	4352	1902	4612	2011
Laboratory: Nov. 24	2611	1141	2017	881
„ „	3482	1521	3991	1744
Average................	3772	1648	3253	1486

There is one very irregular result, probably some accident; but I had no proof of such, and it is therefore retained.

Hydrochloric Acid (Chlorides) in the air.

Devonshire Street, Manchester, 1876.	By shaking.		Aspiration, 3 bottles.	
	Grammes per million cub. metres.	Grains per million cub. feet.	Grammes per million cub. metres.	Grains per million cub. feet.
Yard: Nov. 10, clear afternoon	779	340	206	90
„ „ 11, fine, frosty morning	233	102	51	22
„ „ 13, raining; sheltered from rain	389	170	206	90
„ „ 14, same remarks	389	170	309	135
Laboratory: Nov. 24	273	119	103	45
Average	412	180	175	76

Gases purposely mixed with air.

	By shaking.		Aspiration, 3 bottles.	
	Grammes per million cub. metres.	Grains per million cub. feet.	Grammes per million cub. metres.	Grains per million cub. feet.
Ammonia (till slight smell)............	1866	815	1744	762
„ „	776	339	680	297
Hydrochloric acid	1415	618	1031	450
„ „	1012	442	1010	441

"Free Ammonia" in air.

	By shaking.		Aspiration, 3 bottles.	
	Grammes per million cub. metres.	Grains per million cub. feet.	Grammes per million cub. metres.	Grains per million cub. feet.
Manchester, Dec. 2, 1876. Dull, damp morning	93	41	70	30

"Albuminoid Ammonia" from air washings.

	By shaking.		Aspiration.	
	Grammes per million cub. metres.	Grains per million cub. feet.	Grammes per million cub. metres.	Grains. per million cub. feet.
Manchester, Dec. 2, 1876: dull, damp morning	160	70	53	23
Ditto, Dec. 4: raining	159	69	124	54

I have not, in my usual experiments, separated the solid particles of the air, because we must breathe them. It was thought better to examine them by what is called Wanklyn's process for organic nitrogen. My reason for that is given elsewhere. I never did receive it as a method of obtaining all the nitrogen in every substance; but I did receive it, and do so still, as a method of obtaining the ammonia of that which is easily decomposed; and only such substances are capable of producing fermenting disease according to theory.

No other person, so far as I know, has entered on this part of the subject; and it is the more interesting, as I have sufficiently shown that, in the places examined, the organic ammonia has been in intimate relation with the gross death-rate. At the same time I do not find by this method the form of organic matter in the air. It may be true that oxygen is the prime mover, in man producing animal life—a favourite idea for a chemist; but it may also be true that minute organisms cause a peculiar class of decomposition connected with mental or other activity, diseased or otherwise.

But my present object is explained.

III. "On *a* Cause for the Appearance of Bright Lines in the Spectra of Irresolvable Star-Clusters." By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope. Received August 13, 1877.

In the 'Proceedings' of the Royal Society for 1877, April 26, there is a short paper by Mr. Huggins, in which some views of mine on this subject are called in question. Mr. Huggins's objections are threefold:—

1. "There are not found in the spectra of different nebulæ the differences of relative brightness of the bright lines and of the continuous spectrum which would be expected on Mr. Stone's hypothesis.

2. "The star-clusters which are just within the resolving-power of the largest telescope do not give, even faintly, a spectrum of bright lines.

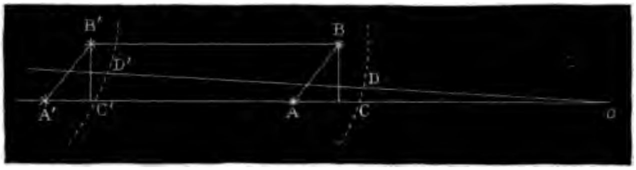
3. "The same bright lines appear to be common to all the nebulæ which give a bright-line spectrum. On Mr. Stone's view, differences in the constitution of the enclosing atmospheres of different star-groups would be probable."

Upon the first point I must remark that, after an examination of the spectra of the nebulæ of Orion and η Argûs, I am not of Mr. Huggins's opinion.

The second point appears to me confirmatory of my views. The irresolvability that I contemplate, as connected with bright-line spectra, does not arise from the sensible vanishing of the angles subtended by the distances between the isolated stellar masses of the cluster. In this case the spectra would be continuous. In my paper in the 'Proceedings' for April 19, I state this point as follows:—"The linear spectrum can only appear when the resolvability of the cluster is at least injuriously affected by the light of the gaseous envelopes becoming sensibly proportional to that from the stellar masses, and in the majority of such cases it would only be in the light from the irresolvable portions of the cluster that bright lines could be seen in the spectrum." By irresolvability I here mean something independent of the power of any particular telescope used.

With respect to Mr. Huggins's third objection I must remark that the difficulty is neither greater nor less, whether my views be accepted or rejected. It cannot be more difficult to conceive that the same gas is present and predominant in certain isolated stellar clusters than that it is present and predominant in certain isolated irregular masses.

With respect to the objection which appears to have been raised by Professor Stokes, "that in a star-cluster in which the stars are surrounded by self-luminous atmospheres, the proportion between the sum total of the light from the stars and the light from the atmospheres will be independent of the distance of the cluster from us." Such, no doubt, would be true in the kind of clusters Professor Stokes has in contemplation. I have, rather unfortunately, in my paper of April 19, spoken of a "close stellar-cluster," without, perhaps, explaining with sufficient clearness that the comparative closeness to which I refer is not optical but linear. The clusters I contemplate are only close in the sense that the individual stellar masses must be close enough to destroy the isolation of the vaporous surroundings, and thus give rise to a gaseous envelope continuous over the whole or over portions of the stellar cluster. I am quite aware, theoretically, as well as from experiment, that the spectra of many clusters, however much they may be condensed to a centre, must be continuous and not linear. In fact, according to my views, the question whether a cluster which was irresolvable with our present optical means would give a continuous or linear spectrum, would depend on whether the irresolvability resulted from the vanishing of its linear dimensions, or from the light of the stellar masses in any given direction having become sufficiently enfeebled in comparison with the light of the intercepted area of the gaseous envelope. The continuousness of the gaseous envelope I contemplate is a physical, not a mere optical, continuity.



Let A and B be two of the stars of such a cluster, O D a section of the dispersed gaseous envelope enveloping the cluster.

Then the light falling within the solid angle at O may be considered to arise from the star A and the area C D of the gaseous envelope.

Suppose the system removed to a greater distance. The light from the star A is diminished in the proportion of the inverse square of the distances, while that from C D is sensibly equal to that from C'D'. The question, therefore, whether such a cluster would, according to my views, end in becoming a nebula or a mere optical cluster, would depend upon whether it would be possible, with any supposed distribution of stellar masses and vaporous envelopes, to diminish the brightness of the star A below that given by the intercepted area C D of the enveloping surface, before the star B has been brought to strengthen the beam of light which gives the continuous spectrum. In the one case we should have absolute irresolvability at that and all greater distances with any optical means at our disposal; in the other irresolvability, which might become resolvability with increase of optical power. The case of two stars is of course only taken for simplicity; it is the proportional increase in other cases which has to be considered. I have chiefly had in contemplation nebulæ like those of Orion and η Argûs, which extend over large angular distances; but at great distances such nebulæ might assume the character of planetary nebulæ. I see no difficulty in conceiving stellar clusters such as those I contemplate, which would give rise to bright-line spectra; and I believe that the more the matter is examined the larger will be the number of facts which will be found to group themselves around the hypothesis which I have suggested.

IV. "Experimental Researches on the Electric Discharge with the Chloride-of-Silver Battery.—Part I." By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. Received August 23, 1877.

(Abstract.)

In the Journal of the Chemical Society, November 1868, we first published an account of the "Chloride-of-Silver Battery." Since 1874 we have commenced working with it systematically, and have gradually augmented the number of cells; we now possess 8040 in actual work, and have 2680 more completed, but not charged with fluid. Amongst the 8040 cells now in use are the first 1080 constructed in 1874, experiments with which we described on the 24th February, 1875*. Subsequently from time to time we have communicated to the Society some of the results we have arrived at, and in the detailed communication of which the present is a short abstract we have given the full particulars of our experiments. The paper in question deals mainly with the striking-distance between terminals of different forms in air and in other gases at ordinary atmospheric pressures, and in air at reduced pressures short of the partial vacua of the so-called vacuum tubes. Besides these experiments the paper describes the effects of currents of high tension in inducing secondary currents, and also their effects in inducing magnetism.

We have found that the discharge of the battery, with one or two poles in the form of a point, presents several interesting phenomena which precede the true jump of the spark, and which do not occur with other forms of terminals—for example, disks or spherical surfaces. With 8040 cells the striking-distance between a paraboloidal point, positive, and a disk is about 0·34 in. (8·64 millims.); but there is always a luminous discharge, very apparent, far beyond the distance measurable by our micrometer-discharger, namely 1·16 inch (29·5 millims.), as we have before stated†.

The current which passes during the luminous discharge which precedes the jump of the true spark is extremely feeble in comparison with that which takes place after the spark has passed and the voltaic arc has formed; even when the point and disk are not more distant than ·02 inch beyond the striking-distance (0·34 inch) for 8040 elements, it is only $\frac{1}{2564}$ part of it; moreover the current is diminished to $\frac{1}{150000}$ of that of the arc when the point and disk are 1·16 inch distant.

The appearance of the discharge is very different, according as the point is positive or negative; it is intermittent in both cases, but is much less discontinuous when the point is negative than when it is positive, as can be seen with a microscope having a rotating mirror placed in the bend of the body, between the objective and eyepiece. The appearances observed are shown in the wood engravings which illustrate the paper.

When a point and a disk 1·5 inch diameter are used as terminals, and a band of glazed writing-paper 1·5 inch wide, and say 0·00425 inch thick, is placed on the disk, a very strong adhesion of the paper to the disk takes place, and it requires a very strong pull, when 8040 cells are employed, to make the paper slide on the disk; the adhesion is strongest when the point is negative. The strain required to make the paper slide on the disk was 30,000 grains with the point negative, and 18,000 grains when it was positive: to reproduce these strains the paper had to be loaded with 129,600 and 53,530 grains respectively after the current had been shut off.

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* Proc. Roy. Soc. vol. xxiii. p. 356.

† Proc. Roy. Soc. vol. xxiv. p. 169.

When terminals with plane or spherical surfaces are used, the luminous phenomena preceding the jump of the spark do not take place, and there is scarcely an appreciable adherence between the band of paper and the terminals; it generally takes a diagonal position, forming a bridge between them.

Between a point and a disk the spark is longest with the point positive, when from 5000 to 8000 cells are used; but for a less number of elements, 1000 to 3000, it is longest when the point is negative.

The length of the spark is greatly influenced by the form of the point: thus with a point in the form of a cone of 20 degrees the striking-distance is 0·184 inch with 5640 cells and 0·267 inch with 8040; while with a point approaching a paraboloid in form, and with the same base and of the same height as the cone, it is 0·237 inch with 5640 cells and 0·343 inch with 8040. The ratios $\frac{184}{237}=0{\cdot}776$ and $\frac{267}{343}=0{\cdot}778$, almost identical, represent the proportion which exists between the length of spark obtained with a conical point and one paraboloidal in form.

The striking-distance between a point and a plate is in accordance, very nearly, with the hypothesis of this distance increasing in the direct ratio of the square of the number of elements, at all events up to 8040 cells, thus*:—

Number of cells ...	1000	2000	3000	4000	5000	6000	7000	8000
	in.	in.	in.	in.	in.	in.	in.	in.
Distance observed...	0·0051	0·0221	0·0554	0·103	0·159	0·222	0·286	0·352
Distance calculated	0·0055	0·0220	0·0495	0·088	0·1375	0·198	0·2695	0·352

Between plane, spherical, or cylindrical surfaces the striking-distance does not follow this law; on the contrary, the increase is nearly, but not quite, in the ratio of the number of cells. We have given the striking-distance from 1000 to 8000 volts, between spherical surfaces 1·5 inch diameter and 3 inches radius, in the Proceedings, No. 182, so that it is only necessary here to state the striking-distance for

	1000 cells.	8000 cells.
	in.	in.
Between spherical surfaces	0·0050	0·0810
„ plane surfaces	0·0104	0·0852
„ two concentric cylinders .	0·0071	0·0991

It must, however, be stated that very probably the striking-distance for 1000 cells between plane surfaces is too great, on account of the difficulty of keeping them absolutely parallel when the micrometer is adjusted from time to time.

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* Proc. Roy. Soc. 1876, vol. xxiv. p. 167.

The striking-distance between two paraboloidal points was found to be with :—

1080 cells.	8040 cells.
in.	in.
0·005	0·401

The nature of the metal used for terminals has, in almost all cases, no influence on the length of the spark; but there is one striking exception, namely, in the case of aluminium. When an aluminium point is used the spark is longer than with points of all other metals tried in the ratio of 1·242 to 1.

The length of the spark is different in various gases—for example, air, oxygen, nitrogen, hydrogen, and carbonic acid; and the ratio between the lengths of spark in various gases varies with the forms of the terminals. The length of the spark bears no simple relation either to the density of the gas or its viscosity*.

The paper contains an account of a few experiments on the length of spark in air at different pressures, from 141·5 millims. to 760 millims. Between a point and a disk the length of the spark increases nearly, but not quite, in the ratio of the dilatation; but between two spherical surfaces it increases far more rapidly, and it is possible that at a certain degree of rarefaction the striking-distance may be coincident for spherical surfaces and points.

The appearance of the voltaic arc at ordinary pressures differs in different gases.

In air (possibly also in other gases) the arc, when examined with the microscope, presents a stratified appearance, especially in the barrel-shaped surrounding of the central brilliant spindle. The striæ are very close, and can be seen with difficulty even when the microscope, with a rotating minor, is employed for the examination of the arc.

In hydrogen, with the point positive, the central spindle of the arc is surrounded with a beautiful blue halo resembling a glass shade illuminated with fluorescent light. With the point negative the arc moves about rapidly and forms a sort of star on the positive disk. Before the jump of the spark, when the point is negative, the luminous discharge has the appearance of a pale olive halo, in form like a glass shade, extending from the point to the periphery of the disk.

In nitrogen the arc is reddish violet; in oxygen it presents the same appearance as in air.

When a strong resistance is interposed in the circuit, 4,000,000 ohms for example, the discharge is completely changed in character; instead of the ordinary spark and production of the voltaic arc, very brilliant snapping sparks pass between the terminals at more or less rapid intervals, exactly like the sparks of a small Leyden jar. They pierce writing-paper with minute holes. It is usually necessary to approach the terminals to a distance of 0·3 inch, when the striking-distance of 8040 cells is 0·34 inch, in order to produce this static discharge.

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* Proc. Roy. Soc. vol. xxvi. p. 227.

It has been found that an accumulated charge of a condenser of 42·8 microfarads capacity, charged with the potential of 3240 cells, produced neither an elongation nor a contraction of a metallic rod 0·2 inch when suddenly discharged through. This charge deflagrates 10·5 inches of platinum wire 0·0125 inch in diameter.

More dense sparks were obtained with one of Apps's coils for producing 6-inch sparks when the primary was connected with 1080, 2280, 3480 chloride-of-silver cells, than when it was used with a zinc-carbon bichromate-of-potash battery of 6 cells, producing a current 300 times as great, thus showing the influence of high potentials in inducing secondary currents.

These currents of high potentials have also a marked effect in inducing magnetism, when the actual current is taken into account.

The second part of the paper, which is in course of preparation, will deal with the discharge in rarefied gases, in the so-called vacuum tubes.

December 20, 1877.

Dr. ALLEN THOMSON, Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Notes on Supersaturated Saline Solutions." By Charles Tomlinson, F.R.S. Received September 14, 1877.

There is probably no subject in science that is more involved in contradiction than that of supersaturation. All the phenomena connected with it seem to behave differently in the hands of different inquirers, so that the facts affirmed by one writer are simply denied by another; and the same theory which seems to have been disproved by one is again and again brought forward by another.

Take one point by way of example, namely, the nuclear action of bodies in producing the sudden crystallization of a supersaturated saline solution. Ziz, in 1809, stated that not only air, but solids, act best as nuclei when dry: if wet, or boiled with the solution, or thrown into it while hot and allowed to cool with it, they are inactive. Löwel (1850–57) denies that air, whether wet or dry, has any nuclear action; but he admits that solids exposed to the air become active, and that alcohol is always active. Selmi and Goskynski, in 1851, assert that dry air is nuclear, and acts by getting rid of water at the surface, and producing small crystals there which continue the action. This seems to be a revival of Gay Lussac's theory, namely, that air is absorbed at the surface of the solution and precipitates a portion of the salt in the same way that one salt may precipitate another, and this precipitate continues the crystallization. Lieben, in 1854, states that soot is a nucleus, also platinum black whether ignited or not; that pounded glass heated in sulphuric acid produces sudden crystallization, but that platinum sponge and precipitated sulphate of baryta after being heated have no action. Schröder, in 1859, remarks that it is always a matter of chance whether such or such a substance produces crystallization. "Such facts," he says, "singularly increase the difficulty of interpreting theoretically the phenomena of supersaturation." He concludes that the only general rule that can be admitted in the presence of so many opposed and contradictory results is that bodies act on supersaturated solutions only after having been exposed to the air. In 1866 Gernez and Viollette and in 1868 Schiff are satisfied that there is only one nucleus for a supersaturated solution, and that is a salt of the same kind as the one in solution or one isomeric therewith. In 1866 Jeannel opposes this theory of pancrystallography, as he calls it, on the ground that it cannot be supposed that crystals, often of rare salts, are to be found waiting in the atmosphere, ready to enter our flasks as soon as they are uncovered. Pellogio* also, in 1875, "gives proofs that the phenomena of supersaturation are not so simple as the French physicists would imply, namely, that the only nucleus is a salt of the same kind," seeing that some supersaturated solutions, such as those of hyposulphite of soda, acetate of lead, acetate of soda, &c., may be exposed to the air, in places where the air is any thing but still, for fifteen or twenty days without the formation of crystals. He states further that porous bodies are active, such as common sponge, platinum black, iron reduced by hydrogen, and carbon. For example, carbon was raised to a red heat, quenched under mercury, and introduced into a solution of 100 sodic sulphate to 102 of water: it fell to the bottom and disengaged gas for some time; crystallization then set in and spread all through the mass. Viollette, on the contrary, finds that bodies greedy of water and capable of being hydrated, such as the fused sulphates of copper and of iron, and porous bodies recently calcined, such as carbon, have no action on supersaturated saline solutions.

In the midst of all this conflict of testimony, my own results were not likely to escape censure. Gernez and others opposed me; and Professor

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* Rendiconti R. Ist. Lomb., 1 Luglio. References to most of the other authorities are given in previous papers.

Grenfell, some eighteen months ago, joining in the opposition, having told me of his experiments, I asked him to prepare a paper on the subject. In this *, as also in a second paper, he is at variance with all his predecessors. In his second paper he gives a summary of the three principal theories that have been propounded, and goes into minute experimental detail in support of his own objections to them and of his own views. His theory rests on absorption, which, it will be seen above, is not so novel as he supposes it to be.

There is one point upon which all observers, down to the time of Prof. Grenfell, are agreed, namely, that if a supersaturated solution, say of Glauber's salt, boiling in a flask, be tied over with bladder and left until cold, it will, if the bladder be pierced, immediately become solid. Indeed this has long been a common lecture-table experiment; and I thought it a step in advance when I showed that if this experiment be made in the open air of the country, the solution remains liquid. I also exposed solutions to the air of my garden with various results, as I have already had the honour of describing to the Society †. But Prof. Grenfell goes far beyond any thing that I, or previous observers, have done; he exposes these solutions to the air of rooms, places them on various surfaces, handles them, makes mud pies with them ‡, and yet they do not crystallize. It is probable that his minute method of dealing with drops may alter the conditions; but certainly in the air of my house and garden I cannot expose my flasks as he does his drops without producing sudden solidification.

I will give two or three out of a multitude of similar results. Last summer I made a strong solution of sodium sulphate and filtered it into two long-necked globular flasks, which were reboiled and tied over with bladder. They were left during three weeks and remained unchanged. The bladder over one flask was then pierced and the solution immediately crystallized. I took the other flask into the fields and uncovered it; it remained liquid during a long walk, although the wind blew into the neck with such force as to produce a musical note. When the still uncovered flask was brought into the house the solution soon crystallized. I have repeated this experiment with this and other salts with similar results, and it furnishes an argument against the Gernez and Viollette theory.

A strong solution of alum was tied over just after being boiled. It was repeatedly shaken during several days. While resting on the mantleshelf of my study the bladder was pierced, when immediately some amorphous powder at the bottom of the solution began to assume

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* Proc. Roy. Soc. xxv. p. 124.

† Proc. Roy. Soc. xx. p. 41.

‡ In April last, during an interval between heavy showers which had thoroughly washed the mould of my garden, I poured upon it a solution of Glauber's salt (2 to 1) and it crystallized immediately.

shape and grow into fine octahedral forms, increasing from the bottom upwards, with the usual evolution of heat.

Two similar flasks in my study, containing sodium sulphate, were pricked with a pin; one crystallized immediately and the other did not. Next day I enlarged the opening into the second flask to the size of a small pea, and after two or three hours the solution became solid.

Pellogio and others claim for sponge and other absorbent bodies a nuclear action. I immersed three bits of sponge in boiling water, and took them out with forceps and put them in a dinner-plate, which together with the forceps were placed in the kitchen oven. Three flasks, containing sodium sulphate solution (3 salt to 1 water), prepared the day before, were taken into the kitchen, and the bits of sponge, now dry, hard, and warm, were taken up with the forceps and dropped severally into the flasks, which were covered with small beakers. The flasks were put upon the window-ledge in my study. One of the solutions crystallized after eight hours; the other two formed a good deal of the modified salt, in which the sponge became entangled: the second solution crystallized on the third day; the third solution had not crystallized after ten days, although it was repeatedly shaken; the beaker was then taken off, and the solution crystallized within half an hour. In all three cases the sponge soon absorbed a portion of the solution and became thoroughly soaked; the solutions were exposed to considerable cold in the window, the weather being chilly, but the sponge was quite inactive.

Such results as these are certainly in opposition to the absorption theory; but the difficulty in this, as in other parts of the subject, lies in the fact that one observer, in an apparently carefully conducted experiment, finds sponge, charcoal, and other porous bodies to be nuclear, while another equally good observer (M. Viollette, for example) declares them to be inactive. In trying the effect of vapours on these solutions some years ago, a sponge dipped in ether, chloroform, bisulphide of carbon, &c. was held over the surface: whenever the sponge touched the surface crystallization set in; or passing the sponge hastily over the surface the portion adhering to the sponge crystallized immediately, while the solution in the vessel did not.

In repeating Prof. Grenfell's experiments on drops, I find the same spirit of contradiction to prevail. The solutions which with him are so permanent are transitory with me. I found sodic sulphate solutions of various strengths, from six parts of salt to one of water, to one part of salt to one of water, to be sensitive in every room of my house and on every kind of surface on which they were deposited. My method was to boil the filtered solution in a clean flask and then to cover the neck with a small beaker. A couple of dropping-tubes (constantly kept in water, except when in actual use, so as to keep them free from nuclei) were employed for getting a supply of the solution from the flasks. A large drop, made up of 5 or 10 or more single drops, was in this way deposited on the surface to be tried, and around this were arranged a number of single drops. My observation agrees generally with that of Prof. Grenfell, that the large drop is more ready to solidify than the smaller ones; and the reason that I should give for this is, that the larger surface is more likely to catch nuclei from the air than the smaller ones. In the early stage of this inquiry it was thought that the sudden shock produced by the inrush of air when the bladder was penetrated produced crystallization; but Prof. Turner, of University College, London, showed that if the neck of the flask containing the solution were drawn out to a capillary opening the solution could be retained in the flask without becoming solid. Löwel also showed that solutions could be kept longest in ordinary flasks in proportion as their necks were narrow; and another early observer noticed that while a flask with its neck upright led to crystallization, it was less likely to do so if the neck were placed at an angle, and still less so if placed horizontally. This evidently points to the increased difficulty of a nucleus getting into the flask under such circumstances.

I tried a large number of experiments on supersaturated solutions in Prof. Grenfell's manner, not only with those of sodic sulphate, but also of sodic acetate, magnesic sulphate, zinc sulphate, potash alum, ammonia alum, sodic carbonate, and some others. On uncovering the flasks for the purpose of filling the dropping-tube, the solutions in the flasks often crystallized suddenly [*]; and it has not unfrequently happened that the drop of sodic sulphate and of some other solution at the end of the dropping-tube suddenly became solid before it fell, and thus acted as a nucleus to the contents of the tube.

I repeated Prof. Grenfell's experiments with oily surfaces. A drop of oil (olive or niger) rubbed with the finger over a glass plate made the plate powerfully nuclear: drops of sodic sulphate became immediately solid, the large drop swelling up into a well-shaped conical figure with a rounded top.

Experiments with magnesic sulphate (2 to 1 solution) showed this salt in drops to be even more sensitive than sodium sulphate. Drops of zinc sulphate (2 to 1) and of sodium carbonate (3 to 1) soon became solid on glass, paper, and other surfaces, whether in the open air of my garden or in different rooms of my house.

Potash alum solution (2 to 1) in drops crystallized rapidly into an opaque white mass, or into transparent crystals in concentric circles round a central pit. A similar solution of ammonia alum behaved in a similar manner: an opaque white ring formed at the circumference of the drop and gradually closing in on the centre, and drawing matter therefrom, produced a pit-like depression. The large drop of the solution often displayed suddenly beneath the surface a small well-shaped octahedron, and this increased in bulk until the whole solution was solid. The crystal would sometimes appear while the large drop was in the act of being built up; and the solutions in the alum flasks, more frequently than in the other flasks, became solid and had to be reboiled.

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[*] In reboiling, the flask must be kept in motion over the flame so as to melt the salt gradually and not allow one part of the flask to become much hotter than another. As the solution approaches boiling, the flask, being chemically clean, is liable to violent bumpings. These may be prevented by keeping in each solution a fragment or two of charcoal (that from cocoanut-shell being most efficacious), as pointed out in my paper in the 'Proceedings' for 1869, p. 251. That the action of the charcoal is not impaired by use has been shown in these repeated reboilings as well as in the cases given by me in the Phil. Mag. for August 1875.

Sodium acetate in drops solidified both in crystalline lines and also in heaps, these varieties occurring in the same set of drops. The crystalline lines originated generally in a radiant-point, and in some cases two or three such points caused the needles to cross. Sometimes in the open air a speck of soot or a small insect formed the radiant-point. Within doors the carrying of the plate across the room caused several of the drops suddenly to crystallize. It was noticed with this and other saline solutions that the deformed drops crystallized first, while those that formed perfect lenses with sharply cut edges remained liquid for a longer time. Drawing a point across these so as to deform their shape generally led to crystallization; but when a tail thus formed sprang back again and the drop recovered its form there was no crystallization. In general when a point was drawn through a drop, crystallization set in where the point made its exit. Oil of almonds smeared over a plate and polished with the pad of the hand was eminently nuclear.

These experiments with drops were conducted during the week of fine weather commencing on the 26th of June of the present year. I had often noticed during many years past, while conducting experiments on the cohesion figures of liquids, the motions of camphor, creosote, &c. on water and on surface-tension generally, that the phenomena varied greatly from day to day. On dull moist days drops spread out into films sluggishly or not at all; camphor, creosote, &c. would be more or less inert; in short, all surface-tension phenomena would be in a depressed state. Whereas on bright sunshiny days, with a brisk dry air, although the temperature might not be high, some force seemed to be present which exalts all the phenomena of surface-tension. It may be the evaporative force or the crystalline force; but whatever it is it has a powerful influence on the phenomena of supersaturation. As already noticed, the solutions in the flasks frequently became solid, and the drop at the end of the dropping-tube could not be delivered on account of its solidifying. The drops on various surfaces formed well-shaped lenses, and any thing that tended to destroy their symmetry led to crystallization. These facts bear affirmatively on the details already laid before the Society by Prof. Van der Mensbrugghe and myself on the connexion between the surface-tension of liquids and supersaturation *. Although the con-

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* Proc. Roy. Soc. xx. p. 342.

clusions of this paper have been called in question by some of the French authorities on this subject, yet I venture to submit that the foregoing details support our main conclusion, which may be so far modified as to present itself in this form, namely, that during certain states of the atmosphere, if not always, such a connexion really exists.

On the 1st of July rain set in, and the weather during some days was cloudy and unsettled. The effect of this change on some of the solutions was remarkable; but I saw reason to direct my attention specially to the behaviour of the sodic acetate solution. During these wet and unsettled days the solution exposed in drops remained permanently liquid. I found that even when crystals of the salt from a drop that had previously become solid were brought into contact with the liquid drops, they no longer produced crystallization. This led me to suspect that the salt is deliquescent as well as efflorescent in varying states of the atmosphere. Accordingly on placing a known weight (30 grains) of the dry salt on filtering-paper in the scale-pan of a balance I found that it increased in weight from day to day in wet weather, and lost weight in dry. The same experiment was tried repeatedly with the supersaturated solution, of which 30 grains placed in the glass scale-pan became 33 in the course of some hours, and went on increasing from day to day during damp weather; but when dry weather set in it lost in weight, and when the solution crystallized there was a further loss of 6 or 8 grains from evaporation consequent on the heat liberated during solidification.

Here, then, is the explanation of the fact which has excited the surprise of several observers, namely, that supersaturated solutions of sodic acetate may remain liquid for days, and even for weeks together, exposed all the while to the dust of the room. Such a fact has been cited in disproof of the theory of aerial nuclei, and was appealed to by the French chemists as a complete answer to the theory of surface-tension; and it had so powerful an effect on the mind of Professor Van der Mensbrugghe as to lead him to withdraw from all further connexion with that theory.

With a westerly wind, a bright sun, a tolerably high temperature, and a moist air, I have known the drops of sodic acetate to remain liquid during eleven or twelve days, and drops that had previously become solid liquefied again. The point of a needle drawn across the drops, which had thus absorbed water, had no action in inducing crystallization, though a considerable amount of friction was used. This was on glass; on writing-paper each drop, after remaining liquid for some days, had a wet halo round it of considerable width; but it seems that as the paper absorbed water from the drop, the drop was absorbing moisture from the air. On passing the point of a pencil through the drops on paper, so as to draw out a tail, there was no action, and a crystal of the salt placed in the drop was equally inert. In fact the solution was no longer supersaturated. Sodic carbonate remained for days together on paper without crystallizing: the paper became wet and sodden, and all appearance of drops ceased.

All the solutions thus exposed in drops are more or less affected by the state of the atmosphere as to moisture. The drops of the solution of alum instead of becoming solid in a short time, as they do when the air is dry, remain for hours in the liquid state, and then one or more of them would display an octahedral crystal in the midst of the liquid, which was the more conspicuous in a compound drop, or one made up of five or ten single drops. In damp weather I have known the drops to remain liquid during from twelve to twenty hours; and I have no doubt that in the damp days of winter this solution, as well as that of other salts, will not be so ready to crystallize as the drops were during the months of June, July, and August, when these experiments were made.

Drops of sodic sulphate in my bedroom crystallized some of them immediately, and all after a few hours. But in a damp air, after heavy rain, the time was considerably extended. I have even found them to remain liquid much longer on paper than on glass. On the other hand I have found magnesic sulphate solidify more readily on paper than on glass, and plumbic acetate and zincic acetate more readily on glass than on paper. But the facility with which the drops solidify on various surfaces seems to vary from day to day; and hence it does not appear possible to tabulate the salts in the order of their sensitiveness, seeing that the order of one day, or even of one hour, may be quite different from that of another day or hour. Thus on two different days in August equally fine, only on one day the wind was in an easterly, and on the other in a westerly quarter, the order in which the solutions respectively crystallized was as follows:—

Wind easterly.	*Wind westerly.*
1. Sodic sulphate.	1. Alum.
2. Sodic acetate.	2. Magnesic sulphate.
3. Alum.	3. Zincic sulphate.
4. Magnesic sulphate.	4. Sodic sulphate.
5. Zincic sulphate.	5. Zincic acetate.
6. Zincic acetate.	6. Plumbic acetate.
7. Plumbic acetate.	7. Sodic carbonate.
8. Sodic carbonate.	8. Sodic acetate.

The following Table shows the order of crystallization during a southerly wind on a day in August, when heavy showers occurred with intervals of bright sunshine:—

1. Sodic sulphate crystallized within a few minutes on glass, and somewhat later on paper.
2. Ammonia alum: the large drop and several of the small ones were covered immediately with minute octahedra; the action was more rapid on glass than on paper.

3. Potash alum was also singularly active, but more so on glass than on paper.
4. Zinc acetate crystallized almost immediately.
5. Magnesic sulphate crystallized almost immediately.
6. Zinc sulphate crystallized after about half an hour.
7. Sodic carbonate flattened and crystallized by evaporation, first on paper and then on glass.
8. Sodic tartrate similar to 7.
9. Lead acetate became solid after about three hours.
10. Sodic acetate was liquid after forty-eight hours, when the experiment was terminated.

On the table in my study, in which this last experiment was conducted, was a glass plate on which drops of sodic acetate had been deposited fifteen days before—one compound drop surrounded by six single drops, all of which, except two single drops, had crystallized. These two had apparently retained their surface-tension, while the other drops had become more or less deformed. There was heavy rain on the 27th August, and on the morning of the 28th all the drops, which had previously crystallized, were quite liquid. Sodic arseniate solution of the same strength (2 to 1) behaves in a similar manner, absorbing moisture from the air in damp weather, and remaining liquid the while; whereas drops of sodic phosphate solution (2 to 1) become solid after a few hours even in damp weather.

An extreme case of persistence in the liquid form during damp weather is afforded by sodic acetate that has undergone the watery fusion; that is, the salt newly crystallized is melted in a test-tube over a spirit-lamp in its own water of crystallization and so boiled. Drops of this on a glass plate will remain liquid during many hours, and even days, if the wind is in a westerly or southerly quarter. Meanwhile they absorb moisture from the atmosphere, as was proved by balancing 44 grains of this solution; it gained 11 grains in the course of a couple of days, and then crystallized slowly with large crystals and interposed water, as in an evaporating-dish, and without that loss of weight which accompanies the sudden crystallization of a supersaturated saline solution. There is only one exception to this loss of weight that I am aware of, and that is in the case of a salt under the aqueous fusion. This, in suddenly solidifying, becomes very hot; but there is no loss of water from evaporation, because the whole of the water present becomes constitutional water in the salt. For example, 45½ grains of recently fused sodic acetate were poured into a balanced watch-glass while still warm, and the solution formed a resplendent convex lens. In the course of an hour it had gained one grain in weight from the absorption of moisture. While watching it, its bright surface became suddenly clouded: from a point in the circumference crystalline lines diverged, so as quickly to cover the surface like a fan, while an opaque appearance was seen to descend throughout the depth of the mass, very much after the behaviour of the 7-atom sodic sulphate when touched by a nucleus. The watch-glass and the metal pan on which it rested became very hot (the temperature rising, as was afterwards found with the remainder of the solution, from 60° to 129° F.)*, but still with no loss of weight, seeing that the whole of the water of the solution had again become water of crystallization. If there is any water to spare it is driven off by the heat of combination, and there is a corresponding loss in weight.

Some of the drops, when exposed to the air, form pulpy masses, such as those of sodic, zincic, and magnesic sulphates; but by continued exposure they soon develop into hard well-formed crystals. This is, I suppose, the explanation of an effect that I obtained some years ago. A spoon of white metal shaped like a deflagrating-spoon, with its wire run through a flexible porous plug, was lowered into a boiling solution of sodic sulphate: the plug was then fitted into the neck of the flask, which it closed accurately, and the solution was left to cool. The spoon was next drawn up full of the solution and left in the air of the flask. In the course of some days the liquid in the spoon evaporated and left a salt which, on being lowered into the solution, was inactive; but if the spoon containing the salt were first taken out and exposed for a few seconds to the open air and then lowered into .the solution, it was powerfully nuclear. This exposure to the open air or to the air of a room seems to be necessary, in some cases, to the formation of a crystalline salt, either by evaporation or by the action of some other force.

Schröder insists on the powerful action of the air in connexion with the phenomena of supersaturated saline solutions; and Löwel asks, "What is this mysterious action of the air which endows bodies with a nuclear action?" He supposed it to be catalytic in its mode. The French theory supposes the air to act as a carrier of salts of the same kind, which, it says, are alone capable of acting as nuclei. In the course of my experiments the air has had abundant opportunities of becoming charged with salts of the same kind as those I was working with in solution. For example: during all the time that I was weighing the solutions of sodic acetate, so as to note their increase in weight by the absorption of moisture from the air, an evaporating-dish, containing sodic acetate in crystals, was within a foot and a half of my balance; but although for days together the solution caught moisture from the air, it never caught a crystal of the salt from the air. When, however, the wind got round to the north, or to the east, the solutions of this and of other salts crystallized, not only when exposed as drops, but also often in the covered flasks which supplied the drops.

When the air is from a dry quarter the results are tolerably uniform;

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* In my first paper (Phil. Trans. 1868, p. 663) a case is given in which sodic acetate solution in solidifying rose from 14° to 104° F.

when from a damp one they vary, although the conditions are apparently the same. For example, one day last summer, when the wind was westerly, and there was a bright warm sun occasionally obscured by clouds, I placed twelve watch-glasses on a parapet wall in my garden, and poured into them, while still warm from the boiling, a solution of sodic sulphate (2 salt to 1 water). In the course of about twenty minutes four of the solutions crystallized in large crystals of the normal salt, with interposed liquid not supersaturated; four formed thin crystalline needles, which started from a radiant-point as from the action of a nucleus; and the remaining four, which remained liquid, were touched on the surface with a needle dipped in olive-oil, and they crystallized in the pulpy form from the point.

I think the foregoing details go some way to prove that supersaturated saline solutions behave differently among themselves and to different bodies under varying atmospheric conditions. If so, many contradictory details connected with this subject are on the road to reconciliation. A supersaturated saline solution in a closed flask in which the air is saturated with moisture is in a different condition (at least as regards surface-tension and the power of spreading the drop of oil into a film) as compared with a similar flask loosely covered and in the open air on a fine day; so that an oil &c. introduced into the closed flask, as in M. Viollette's and Mr. Liversidge's experiments, may be inactive, while under the other condition the same oil may be active. One fine day in August I placed four flasks containing sodic sulphate solution (2 to 1) in the open air, and let fall from a dropping-tube (just taken out of water and put into ether) a drop of ether on the surface of each. It was striking to see, from the cohesion-figure of the ether, long lines of crystals radiating to the sides and bottom of each flask. A fifth flask of the same solution was treated some days later in my laboratory with a drop of ether; but this was inactive, and the solution did not crystallize until the flask was inverted against my thumb. On a day in September, when the wind was S.E., I let fall into each of four flasks containing a similar solution a drop of paraffine-oil (marked "lubricating"). It formed a well-shaped lens on each surface without any separation of salt; but on gently shaking each flask, so as to break up the lens, there was a sudden crystallization, and the normal salt was produced in all four flasks. Some hours later I repeated the experiment in my study with five flasks of a similar solution. The oil spread upon all five surfaces and the solutions crystallized immediately, the normal salt being again produced. I have hundreds of such cases in my note-books which have supplied material for former papers; but in referring to them no discredit is intended to be cast upon the negative results obtained by other observers who deny that oils, ether, &c. have any nuclear action. I would suggest that in my numerous experiments, carried on during several years, it is not probable that there was always a lurking fallacy in the shape of a crystal of the same kind as that of the solution either floating in the air and waiting to enter the flask as soon as it was uncovered, or lying concealed in the oils, ether, and other bodies which I regarded as nuclei. The varied behaviour of oils under different conditions, as pointed out in my second memoir*, has not been attended to; and because in many experiments in the hands of others oils have been inactive, it has been denied that oils have any action at all on these solutions. I confess that I was not able to reconcile the results as stated by my opponents with my own results, until within the last few months, when I set to work to repeat Professor Grenfell's experiments, during which it became plain to me, what I had not before suspected, that the hygrometric condition of the air has a powerful influence on these solutions; so that under one set of conditions bodies may act as nuclei, while under another set they may be inactive, as in the case already cited, where sodic acetate solution by exposure during damp weather becomes inert even to a crystal of the salt itself. But the exact determination of these conditions can only be settled by a further long and laborious experimental investigation upon which I am now engaged.

II. "Notes on Physical Geology.—No. III. On a new Method of finding Limits to the Duration of certain Geological Periods."

By the Rev. SAMUEL HAUGHTON, M.D. Dubl., D.C.L. Oxon., F.R.S., Professor of Geology in the University of Dublin. Received October 4, 1877.

In the preceding Note I have proved that the elevation of Asia and Europe displaced the axis of maximum inertia through 69 miles†, in the direction of the meridian of the Andes, away from Greenwich. The axis of rotation being thus separated from the axis of figure by 69 miles, or 1°, commenced to revolve uniformly on a right cone round the axis of figure, and would continue to do so for ever, if not prevented by friction.

Astronomers are agreed that the motion of the pole at present is secular and very slow, all traces of wabbling caused by the elevation of Asia and Europe having disappeared.

The object of the present note is to discuss the motion of the axis of rotation, from the period of the elevation of the continent until (by reason of friction) the axis of rotation came again to coincide with the axis of figure; and, if possible, to calculate the absolute length of time that has elapsed from the epoch of elevation.

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* Phil. Trans. 1871, pp. 53, 55, 66. See also Proc. Roy. Soc. 1873, p. 210.

† It is stated, in error, as 207 miles in Note II. (*suprà*, p. 57). All the numerical displacements are printed *three times* their proper values, because, through inadvertence, in integrating equation (8), Note I. page 53, I forgot to divide the numerical coefficient by 3. This error affects all the subsequent coefficients.

The equations of motion are:—

$$\left.\begin{aligned} A dp + (C-A) qr\, dt &= L dt, \\ A dq + (A-C) pr\, dt &= M dt, \\ C dr \qquad\qquad &= N dt, \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1)$$

where

A = moment of inertia round equatorial axis,
C = " " " polar "

$\left.\begin{matrix} p \\ q \\ r \end{matrix}\right\}$ = components of rotation round the axes x, y, z, inside the earth,

$\left.\begin{matrix} L \\ M \\ N \end{matrix}\right\}$ couples of forces acting round axes x, y, z.

First Approximation.

Let there be no disturbing forces, then

$$\left.\begin{aligned} L = M = N &= 0, \\ A dp + (C-A) qr\, dt &= 0, \\ A dq + (A-C) pr\, dt &= 0, \\ C dr \qquad\qquad &= 0. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (2)$$

If we assume

$$p = \rho \cos u,$$
$$q = \rho \sin u,$$

ρ will be the total equatorial component of rotation, and u will be the angle made by the meridian of the axis of rotation, with a meridian supposed fixed in the body. Transforming (2) to the new variables, we find

$$\left.\begin{aligned} &A \rho d\rho = 0, \\ &A du - (C-A) r dt = 0, \\ &C dr = 0. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (3)$$

Integrating, we obtain

$$\rho = \rho_0,$$
$$r = r_0,$$
$$u = n't + \text{const.},$$

where ρ_0, r_0 are the initial values of ρ, r, and

$$n' = \frac{C-A}{A} r_0; \quad \ldots\ldots\ldots\ldots \quad (4)$$

or, substituting for C, A, their values (given in Note I. p. 52),

$$n' = \frac{r_0}{304 \cdot 75}.$$

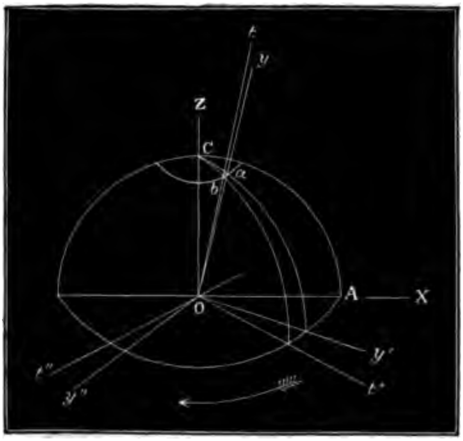
The preceding integrals show that the motion of the instantaneous axis is uniform, and that it moves on the surface of a right cone, whose axis is the new axis of maximum inertia, and that this motion by which the pole revolves uniformly on a circle of 138 miles diameter, completing each revolution in 304·75 days, will continue for ever unless stopped by friction.

In one day the pole moves through 1·423 mile; but while the solid globe (supposed rigid) revolves round each instantaneous axis in succession, it is not so with the ocean, which tends to revolve round yesterday's axis until compelled to revolve round today's axis by the friction of the ocean against its sea-bed. This friction may be thus taken account of.

Second Approximation.

In the accompanying figure, let

OC be the new axis of figure,
OA „ the fixed meridian from which the angle u is measured,
Oy „ yesterday's axis of rotation,
Ot „ today's axis of rotation,
Oy' „ the projection of yesterday's axis on the equator of figure,
Ot' „ the projection of today's axis on the equator of figure,
ab „ the day's path of the pole,
Oy'' „ drawn at right angles to Oy',
Ot'' „ drawn at right angles to Ot',
$u = \mathrm{AO}y'$,
$du = y'\mathrm{O}t'$,
$d\theta = a\mathrm{O}b = y\mathrm{O}t =$ the daily angle described on the surface of the cone.



It is evident that Oy'' is perpendicular to the meridian COy' (yesterday's), and that Ot'' is perpendicular to the meridian COt' (today's).

If ϕ be the angle of separation between the axes of rotation and figure, or semiangle of the cone, we have

$$d\theta=\sin\phi\, du.$$

The solid earth revolves with an angular velocity, ω, round Obt (today's axis), while the ocean revolves with the same rotation round Oay (yesterday's axis).

Let F be a coefficient depending on the friction between the ocean and its bed, and such that, if the ocean be moving relatively to the earth with any angular velocity,

$$\text{F} \times \text{angular velocity} = \text{couple produced} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Resolving the angular velocity of the ocean round Oy (yesterday's axis) into its components, we find the frictional couples

$$\begin{aligned}(1)\quad &\text{Round } OC = F\omega \cos\phi,\\ &\text{Round } Oy' = F\omega \sin\phi.\end{aligned}$$

Resolving the latter into its components, we find

$$\begin{aligned}(2)\quad &\text{Round } Ot'' = F\omega \sin\phi\, du,\\ (3)\quad &\text{Round } Ot' = F\omega \sin\phi \left(1-\frac{du^2}{2}\right).\end{aligned}$$

1. The component of the ocean rotation round OC being the same as that of the solid earth (for yesterday's and today's axes are at the same distance from the axis of figure), there is no frictional couple developed round OC.

2. The frictional couple round Ot'' produces its full effect, for there is no rotation of the solid earth round this axis.

3. The rotation of the solid earth round Ot' is $\omega \sin\phi$, and the component of the ocean rotation falls short of this by the quantity

$$-\omega \sin\phi \frac{du^2}{2}.$$

Hence we have a frictional couple round this axis, retarding the equatorial rotation and equal to

$$-F\omega \sin\phi \frac{du^2}{2}.$$

The frictional component round the axis Ot'' is much greater than that round Ot'; wherefore, neglecting the latter for the present, and resolving the couple round Ot'' round the axes x and y, we have, writing $\rho=\omega \sin\phi$,

$$\begin{aligned}Ldt &= -F\rho \sin u\, du,\\ Mdt &= -F\rho \cos u\, du,\\ Ndt &= 0.\end{aligned}$$

The general equations (1) become, therefore,

$$\left.\begin{aligned} &\mathrm{A}dp+(\mathrm{C}-\mathrm{A})qr\,dt=-\mathrm{F}\rho\sin u\,du,\\ &\mathrm{A}dq+(\mathrm{A}-\mathrm{C})pr\,dt=-\mathrm{F}\rho\cos u\,du,\\ &\mathrm{C}dr=0. \end{aligned}\right\}\quad\dots\dots\quad(6)$$

Transforming p, q into ρ, u, we find, after some reductions,

$$\left.\begin{aligned} &\mathrm{A}d\rho=-\mathrm{F}\rho\sin 2u\,du,\\ &\mathrm{A}du=(\mathrm{C}-\mathrm{A})rdt-\mathrm{F}\cos 2u\,du,\\ &\mathrm{C}dr=0. \end{aligned}\right\}\quad\dots\dots\quad(7)$$

Integrating these equations, and making u, t vanish together, we obtain, as a second approximation to the motion of the pole,

$$\left.\begin{aligned} &\rho=\rho_0 e^{-\frac{\mathrm{F}\sin^2 u}{\mathrm{A}}},\\ &u=n't-\frac{\mathrm{F}\sin u\cos u}{\mathrm{A}},\\ &r=r_0. \end{aligned}\right\}\quad\dots\dots\dots\dots\quad(8)$$

These equations show that a secondary wabble of half the period of the primary wabble is superadded to the motion of the pole, which continues to revolve round the axis of figure in 304·75 days, with the component of rotation round that axis always constant; while the equatorial component has a periodic variation passing through all its changes in 152·37 days; and the velocity of revolution of the primary wabble is not uniform, but also subject to a periodic variation of 152·37 days, because $\sin^2 u$ and $\sin u \cos u$ pass through all their changes in half a revolution.

These results are independent of the magnitude of $\frac{\mathrm{F}}{\mathrm{A}}$, and would hold true even if that coefficient were large, which it is not. The motion of the pole would continue for ever, compounded of the primary and secondary wabbles; and in order to obtain the effects of friction in destroying both motions, it becomes necessary to proceed to the third approximation.

Third Approximation.

We must now introduce into the equations of motion the frictional couple acting round the axis Ot', viz.

$$-\frac{\mathrm{F}}{2}\,\omega\sin\phi\,du^2.$$

Writing for $\omega\sin\phi$ its value ρ, we find

$$\left.\begin{aligned} &\mathrm{A}dp+(\mathrm{C}-\mathrm{A})qr\,dt=-\mathrm{F}\rho\sin u\,du-\frac{\mathrm{F}}{2}\rho\,du^2\cos u,\\ &\mathrm{A}dq+(\mathrm{A}-\mathrm{C})pr\,dt=-\mathrm{F}\rho\cos u\,du-\frac{\mathrm{F}}{2}\rho\,du^2\sin u,\\ &\mathrm{C}dr=0. \end{aligned}\right\}\quad\dots\dots\quad(9)$$

These equations may be transformed, after some reductions, into the following:—

$$\left.\begin{aligned} \frac{\mathrm{A}d\rho}{\rho} &= -\mathrm{F}\sin 2u\,du - \frac{\mathrm{F}}{2}du^2, \\ \mathrm{A}du &= (\mathrm{C}-\mathrm{A})r\,dt - \mathrm{F}\cos 2u\,du, \\ \mathrm{C}dr &= 0. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (10)$$

The second and third of these equations are the same as the second and third of (7), and the first may be integrated as follows:—

The sum of (du^2), taken through an entire circumference, is

$$\Sigma(du^2) = 2\pi\,du.$$

Hence, if we assume the value of ρ to remain constant during a single wabble, and then to change by a small quantity, remaining constant for the next wabble, and so on; if

$$\rho_0,\ \rho_1,\ \rho_2 \ \ldots\ldots\ldots\ldots \ \rho_n$$

be the successive values of ρ, we have, since the periodic term

$$\int_0^{2\pi} \sin 2u\,du$$

destroys itself,

$$\log\left(\frac{\rho_1}{\rho_0}\right) = -\frac{\pi\mathrm{F}\,du}{\mathrm{A}},$$

$$\log\left(\frac{\rho_2}{\rho_1}\right) = -\frac{\pi\mathrm{F}\,du}{\mathrm{A}},$$

&c. &c.;

and, in general,

$$\log\left(\frac{\rho_n}{\rho_0}\right) = -n \times \frac{\pi\mathrm{F}\,du}{\mathrm{A}},$$

or, writing

$$y = \frac{\pi\mathrm{F}\,du}{\mathrm{A}},$$

$$\left.\begin{aligned} &\frac{\rho_1}{\rho_0} = e^{-y}, \\ &\frac{\rho_2}{\rho_1} = e^{-y}, \\ &\text{\&c. \&c.,} \\ &\frac{\rho_n}{\rho_0} = e^{-ny}. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (11)$$

The equatorial component of rotation, ρ, therefore diminishes in geometrical progression, and finally disappears altogether, leaving the earth's rotation round the axis of figure OC equal to r.

If we know the value of y (depending on the friction), we are now prepared to calculate from (11) how long it would take to reduce ρ_0 to ρ_n.

Suppose

$$\frac{\rho_0}{\rho_n}=A'=e^{ny},$$

then

$$\log_e(A')=ny,$$

or

$$n=\frac{\log_e A'}{y}. \qquad (12)$$

This equation determines n, the number of wabbles necessary to reduce ρ_0 to ρ_n, where

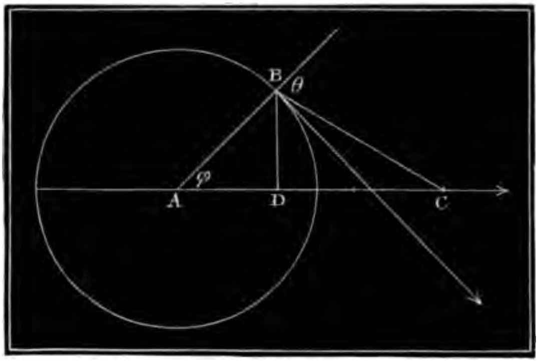
$$\frac{\rho_0}{\rho_n}=A',$$

supposed to be a given quantity.

Let us now proceed to calculate the coefficient of friction from the retardation of the earth's angular velocity produced by the friction of the Tidal Residual Current.

This retardation amounts to one second in the length of the day in 100,000 years*.

The accompanying figure shows Newton's construction for the disturbing force of the moon upon the ocean. The moon lies in the direction AC, and the distance of the moon from the earth (=60 AB) represents the attraction of the earth at the distance of the moon. The disturbing force is BC, if AC=3AD. Let AB=r, then the tangential component of the disturbing force is



$$BC \sin\theta=3r\sin\phi\cos\phi,$$

$$\text{Tangential force}=\frac{3r}{2}\sin 2\phi. \qquad (13)$$

The relative velocity of the tide is thus found,

$$dv=-\frac{3r}{2}\sin 2\phi\, dt.$$

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* Delaunay, 'Sur le ralentissement du mouvement de rotation de la Terre' (Paris, 1866).

If ω be the angular velocity of the earth's rotation,

$$\phi=\omega t,$$

and

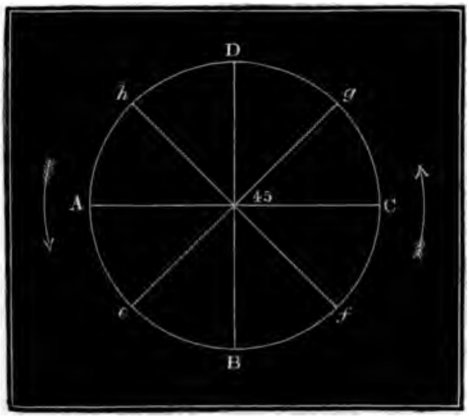
$$dv=-\frac{3r}{2\omega}\sin 2\phi\, d\phi,$$

$$v=\frac{3r}{4\omega}\cos 2\phi. \quad \ldots\ldots\ldots\ldots \quad (14)$$

Remembering that the earth's attraction upon the moon is $\frac{89}{10000}$ ft. per sec., we have

$$\frac{3r}{4\omega}=\frac{3\times 89\times 86400}{4\times 60\times 10000\times 2\pi}=1{\cdot}53 \text{ ft. per sec.}$$

If AC be the line joining the earth and moon, the relative velocity of the tide will be zero at the points e, f, g, h; the maximum easterly velocity will occur at A and C, and the maximum westerly velocity at B and D*. If r were constant these velocities would be equal and opposite, and the earth's rotation would not be affected by them; or, in other words, there would be no Residual Tidal Current; but since in reality the axis BD exceeds the axis AC by some feet, the greatest westerly velocities will exceed the greatest easterly velocities, and thence will result a balance of tidal current retarding the earth's rotation and lengthening the day.



If b, a denote the lesser and greater semiaxes of the disturbed water surface, and

$$e=\frac{a-b}{a},$$

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* From f to g, and from h to e, the water is moving faster than the earth, and from g to h, and from e to f, the water is moving slower than the earth.

we shall have

$$\text{Maximum Westerly Current at B and D}=\frac{3a}{4\omega},$$

$$\text{Maximum Easterly Current at A and C}=\frac{3b}{4\omega},$$

$$\text{Difference}=\frac{3a}{4\omega}\,\epsilon.$$

From (14) we have,

$$vd\phi=\frac{3r}{4\omega}\cos 2\phi\, d\phi\,;$$

but

$$r=a(1-\epsilon\cos^2\phi), \quad \ldots\ldots\ldots\ldots \quad (15)$$

therefore

$$vd\phi=\frac{3a}{4\omega}\cos 2\phi(1-\epsilon\cos^2\phi)d\phi.$$

This is to be integrated all round the circumference, in which process the periodic terms disappear, and we find

$$vd\phi=\frac{3a}{4\omega}\left\{\left(1-\frac{\epsilon}{2}\right)\cos 2\phi d\phi-\frac{\epsilon}{4}\left(1+\cos 4\phi\right)d\phi\right\}\,;$$

and, finally,

$$\int_0^{2\pi} vd\phi=-\frac{3a}{4\omega}\times\frac{\pi\epsilon}{2}. \quad \ldots\ldots\ldots\ldots \quad (16)$$

The mean radius of the earth is

$$3958 \text{ miles}=3958\times 5280 \text{ ft.}=20{,}898240\,;$$

say 21 million ft.; hence, assuming $a-b=3$ ft.,

$$\epsilon=\frac{1}{7{,}000000}\,;$$

therefore

$$\text{Residual Tidal Current}=-\frac{1{\cdot}53}{7{,}000000}\times\frac{\pi}{2}\text{ ft. per second.}$$

The angular velocity of the Residual Tidal Current will therefore be

$$\frac{1{\cdot}53}{7{,}000000\times 21{,}000000}\times\frac{\pi}{2}.$$

The retardation of angular velocity caused by friction is equal to

$$\frac{\text{F}\times\text{time}\times\text{angular velocity}}{\text{moment of inertia}}, \quad \ldots\ldots \quad (5)$$

or

$$\frac{\text{F}}{\text{C}}\times\frac{1{\cdot}53t}{7{,}000000\times 21{,}000000}\times\frac{\pi}{2}.$$

This retardation (according to Adams and Delaunay) amounts to one second in the length of the day during 100000 years; hence we obtain the following equation to determine the value of $\frac{F}{C}$:—

$$\frac{F}{C}\times\frac{1\cdot53\times100000\times365\cdot25\times86400}{7{,}000000\times21{,}000000}\times\frac{\pi}{2}=\frac{1}{86400}.$$

Hence, finally,

$$\frac{F}{C}=\frac{2}{\pi}\times\frac{7{,}000000\times21{,}000000}{1\cdot53\times100000\times365\cdot25\times(86400)^2},$$

which gives

$$\frac{F}{C}=\frac{1}{4457}. \qquad \qquad (17)$$

We have already defined

$$y=\frac{\pi F du}{A}.$$

But du is the daily change of angle in the position of the equatorial axis of rotation, ρ, and is equal to

$$du=\frac{2\pi}{304\cdot75},$$

$$\frac{F}{C}=\frac{1}{4457},$$

$$\frac{C}{A}=\frac{307}{306}.$$

Hence

$$y=\frac{2\pi^2\times307}{4457\times306\times304\cdot75},$$

which gives

$$y=\frac{1}{68{,}588}.$$

We are now in a condition to apply equation (12) to determine how long it would take to destroy a wabble of 69 miles, if suddenly produced.

Our astronomical instruments are now so perfect that an annual displacement of 10 ft. in the pole could be detected. This would correspond with a radius of 5 ft., instead of 69 miles, for our wabble.

Hence

$$A'=\frac{\rho_0}{\rho_n}=\frac{69\times5280}{5}=72{,}864.$$

Hence

$$n=\frac{\log_e A'}{y}=\log_e(72{,}864)\times68{,}588,$$

$$=767{,}940 \text{ wabbles},$$

$$=\frac{304\cdot75}{365\cdot25}\times767{,}940 \text{ years},$$

$$=640{,}730 \text{ years}.$$

From the preceding investigation it appears, if Asia and Europe were manufactured, *per saltum*, causing a sudden displacement of the axis of figure through 69 miles, that this event cannot have happened at an epoch less than 641,000 years before the present time, and that this event may have occurred at an epoch much more remote.

It is highly improbable that the elevation of the nummulitic limestone took place so rapidly; and I shall therefore discuss several cases of supposed slower elevation.

1. Let the displacement of the axis of figure caused by the manufacture of Asia and Europe have been due to 69 geological convulsions, each of which displaced the axis of figure through one mile, and let the radius of the wabble be reduced from one mile to 5 feet in the interval between each two successive convulsions.

From equation (12) we have

The interval between two successive convulsions

$$= \log_e\left(\frac{5280}{5}\right) \times 68{,}538,$$

$$= 477{,}520 \text{ wabbles},$$

$$= 398{,}420 \text{ years}.$$

Total time occupied in the manufacture of Asia and Europe

$$= 398{,}420 \times 69,$$

$$= 27{,}491{,}000 \text{ years};$$

say, 27½ million years.

From this it also appears that no geological change, altering the position of the axis of figure through one mile, can have taken place within the past 400,000 years.

2. Let us suppose that the Earth's wabble has the minimum observable radius of 5 ft., and that geological changes take place at such a rate that the increase of this radius is exactly destroyed by friction during each wabble, so that the radius of 5 ft. continues constant.

We now have

$$A = \frac{5 + z}{5},$$

where z is the displacement of the axis of figure during 305 days.

Equation (12) becomes

$$\log_e\left(1 - \frac{z}{5}\right) \times 68588 = 1,$$

or

$$z = \frac{1}{13718} \text{ ft}.$$

At this rate of geological change the total time required for the manufacture of Asia and Europe would be

$$69 \times 5280 \times 13718 = 4997,600,000 \text{ wabbles,}$$

$$= 4170 \text{ million years.}$$

Assuming the minor limit of the epoch of the elevation of the nummulitic limestone to be 640,000 years, we can assign an approximate minor limit to the whole period during which stratified rocks have been forming on the surface of the globe, assuming the rate of deposition to have been, on the whole, constant.

The following Table gives the approximate thickness of the several geological strata in Europe :—

		feet.
1. Eozoic		26,000
2. Lower Palæozoic	Lower Silurian	25,000
	Upper Silurian	5,500
3. Upper Palæozoic	Devonian	9,150
	Carboniferous	14,600
	Permian	3,000
4. Neozoic	Triassic	2,200
	Jurassic	4,590
	Cretaceous	11,283
	Nummulitic	3,000
	Tertiary	6,000
	Total	110,323

The proportion which the total thickness of strata bears to that of the Tertiary beds is as

110,323 to 6000.

Hence the *minor limit assignable to geological time* is

$$\frac{110323 \times 640730}{6000} = 11,700000 \text{ years,}$$

or 12 million years in round numbers.

It is extremely improbable that the continent of Asia and Europe was formed *per saltum*, and therefore our minor limit of time is probably far short of the reality.

If Europe and Asia were formed in a million years, which is 27½ times as rapid as supposition No. 1, the total duration of geological time would be nearly 37 millions of years.

Whatever may have been the time actually employed in the manufacture of Asia and Europe, that process must have increased the length of day by an amount which may be thus calculated :—

Let ω_0 and r_0 be the original and present angular velocity, and ρ_0 the equatorial component which is destroyed by friction. Then

$$\frac{\omega_0}{r_0}=\sqrt{\frac{r_0^2+\rho_0^2}{r_0^2}}=1+\tfrac{1}{2}\left(\frac{\rho_0}{r_0}\right)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

But

$$\frac{\rho_0}{r}=\tan 1^\circ;$$

therefore

$$\frac{\omega_0}{r_0}=1{\cdot}00015234.$$

Hence the original length of the day was

$$\frac{86400}{1{\cdot}00015234}=86386{\cdot}8 \text{ seconds.}$$

Hence the formation of Asia and Europe had the effect of lengthening the day by 13·2 seconds, no matter at what rate the formation took place.

III. "On certain Movements of Radiometers." By G. G. Stokes, M.A., Sec. R.S. Received November 1, 1877.

Nearly two years ago Mr. Crookes was so good as to present me with two of his beautiful radiometers of different constructions, the disks of one being made of pith, and those of the other of roasted mica, in each case blackened with lampblack on one face. With these I was enabled to make some experiments, having relation to their apparently anomalous movements under certain circumstances, which were very interesting to myself, although the facts are only such as have already presented themselves to Mr. Crookes, either in the actual form in which I witnessed them, or in one closely analogous, and have mostly been described by him. Although it will be necessary for me to describe the actual experiments, which have all been repeated over and over again so as to make sure of the results, I do not bring forward the facts as new. My object is rather to endeavour to coordinate them, and point to the conclusions to which they appear to lead.

I do not pretend that these conclusions are established; I am well aware that they need to be further confronted with observation; but as I have not leisure to engage in a series of experiments which would demand the expenditure of a good deal of time, and have lately been urged by a friend to publish my views, I venture to lay them before the Royal Society, in hopes that they may be of some use, even if only in the way of stimulating inquiry.

In describing my experiments I will designate that direction of rotation in which the white face precedes as positive, and the reverse as negative.

It will be remembered that, under ordinary circumstances, radiation towards either radiometer produces positive rotation.

1. If a glass tumbler be heated to the temperature of boiling water, and inverted over the mica radiometer, there is little or no *immediate* motion of the fly; but quickly a *negative* rotation sets in, feeble at first, but rapidly becoming lively, and presently dying away.

2. If after the fly has come to rest the hot tumbler be removed, a *positive* rotation soon sets in, which becomes pretty lively, and then gradually dies away as the apparatus cools.

3. If the tumbler be heated to a somewhat higher temperature, on first inverting it over the radiometer there is a *slight positive* rotation, commencing with the promptitude usual in the case of a feeble luminous radiation, but quickly succeeded by the negative rotation already described. If the tumbler be heated still more highly, the initial positive rotation is stronger and lasts longer, and the subsequent negative rotation is tardy and feeble.

4. If the pith radiometer be treated as in § 1, the result is the same, with the remarkable difference that the rotation is positive instead of negative; it is also less lively.

5. But if the tumbler be removed when the fly has come to rest, it remains at rest, or nearly so.

6. If the tumbler be more strongly heated, positive rotation begins as promptly as with light. In this case the tumbler must not be left long over the radiometer, for fear the vacuum should be spoiled by the evolution of gas from the pith.

7. If the tumbler be heated by holding it over the spout of a kettle from which steam is issuing, and held there till the condensation of water has approximately ceased, and be then inverted over the pith radiometer, the bulb is immediately bedewed, and a *negative* rotation is almost immediately set up, though sometimes, just at the very first moment, there is a trace of positive rotation. The negative rotation is lively, but not lasting; and, after 15 seconds or so, is exchanged for a positive rotation, which is not lively, but lasts longer.

8. If the tumbler be lifted when the negative rotation has ceased, and the dewed surface be strongly blown upon, a lively, but brief, positive rotation is set up.

9. To produce positive rotation by blowing, it is not *essential* that the bulb be wet. If it be merely warm, and the circumstances are such that the fly is at rest for the moment, or nearly so, blowing produces positive rotation, though much less strongly than when the bulb is wet.

10. If the tumbler be heated as in § 7, and inverted over the mica radiometer, the rotation is positive, as when the tumbler is dry.

11. If the tumbler or a cup be smoked inside (to facilitate radiation), heated to a little beyond the temperature of boiling water, and inverted over the pith radiometer, a positive rotation is produced; and if, when

this has ceased, which takes place in a couple of minutes or so, the heated vessel be removed, a negative, though not lively, rotation is produced as the apparatus cools.

12. These results do not seem difficult to coordinate so far as to reduce them to their proximate cause.

As regards the small quantity, if any, of heat radiated directly across the glass of the bulb, the action of which was experimentally distinguishable by its promptitude, both radiometers behaved in the ordinary way.

13. As regards the mica radiometer, when the bulb gets heated, and radiates towards the fly, the fly is impelled in the negative direction, *as if* the white, pearly mica were black and the lampblack were white. And there is nothing opposed to what we know in supposing that such is *really* their relative order of darkness as regards the heat of low refrangibility absorbed and radiated by the glass; for the researches of Melloni and others have shown that lampblack is, if not absolutely white, at any rate very far from black as regards heat of low refrangibility. On the other hand, glass and mica are both silicates, not so very dissimilar in chemical composition; and it would not therefore be very wonderful, but rather the reverse, if there were a general similarity in their mode of absorption of radiant heat, so that the heat most freely radiated by glass, and accordingly abounding in the radiation from *thin* glass such as that of the bulb, were greedily absorbed by mica. The explanation of the reversal of the action when heat and cold were interchanged is too well known to require mention.

14. With the pith radiometer, when the bulb as a whole is heated, and radiates towards the fly, the impulse is positive, though less strong than in the case of the mica (§ 4); and when the bulb as a whole is cooler than the fly the impulse is negative (§ 11).

But to explain all the phenomena we must dissect the total radiation from or towards the bulb. When I first noticed the negative rotation produced by a heated wet tumbler, I was disposed to attribute it to radiation from the water, which possibly the glass of the bulb might be thin enough to let pass; but when I found that hot water in a glass vessel outside, even though the glass of it were thin, produced no sensible effect, and that blowing on the heated bulb when it was dry produced a similar effect to blowing on it when dewed, though of much less amount, I perceived that the moisture acted, not by direct radiation from it, and in consequence of a difference of quality between the radiations from glass and water, but by causing a rapid *superficial* heating of the bulb; and, similarly, the blowing on the dewed surface acted by causing a rapid superficial cooling. When the dry tumbler radiates to the bulb, the radiation is absorbed at various depths; the absorption is most copious, it is true, at the outer strata; but still the change of temperature is not by any means so much confined to the immediate surface as when we have to deal with the latent heat of vapour condensed on it, or obtained from it by rapid evaporation.

Hence, thin as is the glass of the bulb (about 0·02 in. thick), we must still, in imagination, divide it into an outer and inner stratum, and examine the effects of these separately. The heat radiated by either stratum depends only on its temperature; but the radiation from the outer, on its way to the fly, is sifted by passing through the inner, and the portion for which glass is most excessively opaque is in great part stopped. It appears from the observed results that the residue acts decidedly negatively, while when the bulb is pretty uniformly heated there is positive action. We may infer that if it were possible to heat the inner stratum alone it would manifest a very decided positive action.

15. In the struggle between the opposing actions of the outer and inner strata we see the explanation of the strange behaviour of the pith radiometer. In the experiment of § 7 the outer stratum at first shows its negative action; but quickly the inner also gets heated, partly by conduction from the outer, partly by direct radiation from the tumbler, and then the inner prevails. In the experiment of § 5 the whole bulb cools, partly by radiation, partly by convection, while the fly remains warmer; and the slightly greater coolness of the outer than of the inner stratum makes up for the superiority of the inner when the two are equally cool, so that the antagonistic actions nearly balance, and slight causes, such as greater or less agitation of the air, suffice to make the balance incline one way or other. That the inner stratum *would* prevail if the two were about equally cooled may be inferred from the behaviour of the radiometer when the bulb is pretty uniformly heated (§§ 4, 11), or shown more directly by cooling the bulb with snow, when a negative rotation may be obtained.

16. The complete definition of a radiation would involve the expression of the intensity of each component of it as a function of some quantity serving to define the quality of the component, such as its refractive index in a standard medium, or its wave-length, or the squared reciprocal of the wave-length*. The experimental determination of the character, as thus defined, of a radiation consisting of invisible heat-rays is beset with difficulties, at least in the case of heat of extremely low refrangibility; and in general we can do little more than speak in a rough way of the radiation as being of such or such a kind. It is obvious that the behaviour of radiometers by itself alone affords no indication of the refrangibilities of the kinds of heat with which we have to deal; nevertheless, by combining what we know of the behaviour of bodies in respect

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* A map of the spectrum, constructed with the squared reciprocals of the wave-lengths for abscissæ, would be referred to a natural standard, no less than that of Ångström, which is constructed according to wave-lengths; while it would have the great advantage of admitting of ready comparison with refraction spectra, the kind almost always used.

to radiations in general (especially luminous radiations, which are the most easily studied) with what we observe as to the motions of radiometers, we may arrive at some probable conclusions.

17. We may evidently *conceive* a series of ethereal vibrations of any periodic time, however great, to be incident on a homogeneous medium such as glass, and inquire in what manner the rate of absorption would change with the period; though whether we can actually *produce* ethereal vibrations of a very long period is another question, seeing that we can only act on the ether by the intervention of matter, and are limited to such periods of vibration as matter can assume when vibrating molecularly, in a manner communicable to the ether, and not as a continuous mass, in the manner of the vibrations which produce sound. We may inquire whether, on continually increasing the period of vibration, the glass (or other medium) would ultimately become and remain very opaque, or whether, after passing through a range of opacity, it would become transparent again, on still further increasing the period of the incident vibrations.

18. This is a question the experimental answer to which, as it seems to me, could only be given, in so far as it could be given at all, as a result of a long series of experiments, of a kind that Melloni has barely touched on. A variety of considerations, which I could not explain in short compass, lead me to regard the second alternative as the more probable, namely, that, on increasing the periodic time, homogeneous substances in general (perhaps even metals, though this is doubtful) become at last transparent, or at least comparatively so. The limit of opacity, in all probability, varies from one substance to another; and the lower it is, the lower would be the lowest refrangibility of the radiation which the same substance is capable of emitting.

19. In what immediately follows I shall suppose accordingly that glass is strongly absorbing through a certain range of low refrangibility, on *both* sides of which it gradually becomes transparent again*. Imagine a spectrum containing radiations of all refrangibilities with which we have to deal; let portions of this spectrum on the two sides of the region of powerful absorption for glass be called *wings* of that region, and let left to right be the order of increasing refrangibility. Then the spectrum of the radiation from a thin plate of glass, if it could be observed, would be seen to occupy the region of chief absorbing (and therefore emitting) power and its wings. The spectrum of the radiation from the outer stratum of the bulb of the pith radiometer, after transmission through the inner, would consist of two wings, with a blank, or nearly blank, space between; it would resemble, in fact, a widened bright spectral line, with

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* It may be noticed that this supposition, which, as appearing the more probable, is adopted for clearness of conception, is not essentially involved in the explanation that follows, which would hardly be changed if the "left wing" were not terminated on the left.

a dark band of reversal in its middle, save that, instead of being confined to extremely narrow limits of refrangibility, the central space and its wings would be of wide extent. It follows from the experiments that, in the complete radiation from glass, the portions of the spectrum called the wings together act negatively, the portion between positively. It does not, of course, follow that each wing acts negatively, but only that the balance of the two is negative. When the tumbler is heated a little over 212° there is a slight positive action from radiation which passes directly through the bulb. The circumstances lead us to regard this as an extension of the right wing; for it comes from a depth, measured from the inner surface of the bulb in glass, *i. e.* not counting the intervening air, somewhat greater than the thickness of the wall of the bulb; and we know that the more a solid body is heated, the higher, as a rule, does the refrangibility of the radiation which it emits extend, and the greater the proportion of rays of high to those of low refrangibility. It is simplest, therefore, to suppose that the action of the right wing, like that of the space between the wings, is positive, and that the observed negative action in the experiment of § 7 is due to the excess of negative action of the left wing over positive action of the right. In the mica radiometer the experiments indicate no such difference of action in the different layers of the bulb as in the case of the pith radiometer. Hence taking, in accordance with what now appears to be made out to be the theory of the motion of the radiometer, the direction in which the fly is impelled as an indication which is the warmer of the two faces of the disks, and that again as an indication which is the darker with respect to the radiation to which it is exposed, we arrive at the following results as regards the order of darkness of the substances for the three regions into which the spectrum of the incident radiation has been supposed to be divided, the name of the lighter substance being in each case placed above that of the darker:—

	Left wing.	Region of intense absorption by glass.	Right wing.
From pith radiometer	Lampblack. Pith.	Pith. Lampblack.	Pith. Lampblack.
From mica radiometer	Lampblack. Mica.	Lampblack. Mica.	Mica. Lampblack.

Hence, on descending in refrangibility, the order of darkness of the two substances of either pair is at first the same as for the visible spectrum, and at last the opposite; and the reversal of the order takes place sooner with mica and lampblack than with pith and lampblack. The order falls in very well with that of the chemical complexity of the three substances.

20. The whole subject of the behaviour of bodies with respect to radiant heat of the lowest degrees of refrangibility seems to me to need a thorough experimental investigation. The investigation, however, is one

involving considerable difficulty. We can do little towards classifying the rays with which we are working unless we can form a pure spectrum. A refraction-spectrum is the most convenient; but the only substance known which would be approximately suitable for forming the prism, lens, &c. required for such a spectrum, and for confining liquids, is rock-salt, of which it is extremely difficult to procure perfectly limpid specimens of any size; and even rock-salt itself, as Professor Balfour Stewart has shown, is defective in transparency for certain kinds of radiant heat. Then, again, the only suitable measuring-instrument for such researches, the thermopile, demands a thorough examination with reference to the coating to be employed for absorbing the incident radiation. Hitherto lampblack has been used almost exclusively for the purpose; and it is commonly assumed, in accordance with certain of Melloni's results, that lampblack absorbs equally heat-rays of all kinds. But the experiments by which Melloni established the partial diathermancy of lampblack prove that rays exist for the absorption of which that substance is unsuitable.

On calling on Mr. Crookes after the above was written, I was surprised to find that all his mica radiometers behaved towards a heated glass shade in the opposite way to that he had given me, going round positively instead of negatively. Mr. Crookes showed me and gave me a specimen of the kind of mica he employs, as eminently convenient for manipulation. It is found naturally in a condition resembling artificially roasted mica. It is not, however, quite so opaque for transmitted light, nor of quite such a pearly whiteness for reflected light as that which has been artificially roasted at a high temperature. The mica radiometer that Mr. Crookes first gave me, which I will call M_1, was, Mr. Gimingham told me, the only one they had made with roasted mica.

Mr. Crookes was so kind as to give me, for comparative experiment, a mica radiometer, which I will call M_2, made from the natural foliated mica. It revolves a good deal more quickly than M_1 under the influence of light; it also gets more quickly under way, indicating that the mica is thinner. When covered with a hot glass it revolves positively, as already remarked; there is, however, but little negative rotation when the glass is removed.

The difference in the thickness and condition of the mica sufficiently explains the difference of behaviour of M_1 and M_2. Any radiant heat incident on the white face that reaches the middle of the mica, whether it afterwards is absorbed by the mica or reaches and is absorbed by the lampblack, tends to heat the second or blackened face more than the first, and therefore conspires with the heat incident on the lampblack, and absorbed by it, to produce positive rotation; and the smaller thickness and less fine foliation of the natural mica are favourable to the transmission of radiant heat to such a depth.

P.S.—It might be supposed at first sight that the change of rotation from negative to positive (in § 7) was due, not to a change in the conditions of absorption, but to the circumstance that the inner surface of the bulb had become warm by conduction, so as to be warmer than the surfaces of the fly instead of colder. For we now know that the "repulsion resulting from radiation," as in some way or other it undoubtedly does result, is an indirect effect, in which radiation acts only through the alterations it occasions in the superficial temperatures of the solids in contact with the rarefied gas; and it might be supposed that when the inner surface of the bulb passed from colder than the fly to warmer, the direction of rotation would, on that account alone, be reversed. This, however, is not so. If bulb and fly are at a common temperature, and the instrument is protected from radiation, the fly remains at rest whether the common temperature be high or low. If a small portion of the total surface in contact with the rarefied gas be warmed by any means, repulsion takes place, through the intervention of the rarefied gas, between the warmed surface and the opposed surfaces, if not too distant; if it be cooled, the result is attraction. It does not matter whether the surface at the exceptional temperature belong to the fly or the bulb. The former takes place in the ordinary case of a radiomter exposed to radiation, the latter in that of a radiometer at a uniform temperature and protected from radiation when a small portion of the bulb is warmed or cooled, in which case the part at the exceptional temperature repels or attracts the disk irrespectively of its colour or the nature of its coating*. Suppose now that the fly is being warmed by radiation from without, the bulb being cool, at least at its inner surface. Let A, B be the two kinds of faces of the disks, and suppose A to be the better absorber of the total radiation. Then A will be the warmer, and therefore will be more strongly repelled than B. Suppose now that the bulb is heated till its inner surface becomes warmer than the fly. Then the fly will still be receiving heat by radiation, to some extent also by communication from the gas; but this will be the same for both faces. Hence if A be still the better absorber of the two (A, B), A will be the warmer, and being less below the temperature of the interior surface of the fly will be less attracted, or, which is the same, more repelled. Hence, whether the inner surface of the bulb be cooler or hotter than the fly, a reversal in the direction of rotation, while the fly is being heated, indicates a reversal in the order of absorbing power of the two faces, and that, again, shows that the order is different for different components of the total radiation, and that the ratio of the intensity of those components has been changed.

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* Theoretically there would be a minute difference of temperature, produced, other circumstances being alike, by the difference in the absorbing or emitting power of the two faces of a disk, as regards the radiation which is the difference between the radiations from or towards the affected portion of the bulb and the same portion at the normal temperature. But this, and the repulsion or attraction corresponding to it, would be only a small quantity of the second order, the main effect being deemed one of the first order.

It is perhaps hardly necessary to observe that the radiometers mentioned in this paper are of the usual form—that is to say, that their arms are symmetrical, so far as *figure* is concerned, with respect to a vertical plane passing through the point of support. Accordingly the rotation which is attained, for instance, with a radiometer with concave disks of aluminium, alike as to material on both faces (of which kind, again, I owe a beautiful specimen to Mr. Crookes's kindness), has not been referred to. This rotation, depending on the more favourable presentation, to the bulb, of the outer (and therefore nearer and more efficient) portions of the fly on the convex than on the concave side, has nothing to do with the one isolated subject to which the present paper relates, namely, the elucidation of the peculiar behaviour in certain cases of certain kinds of radiometers, by a consideration of the heterogeneous character of the total heat-radiation.

November 20, 1877.

2nd P.S.—This morning I received from Mr. Crookes an account of the behaviour of a kind of radiometer which he was so good as to construct at my suggestion. The consideration of an experiment mentioned in a paper of his presented to the Royal Society, which will shortly be read, and which he has kindly permitted me to refer to, suggested to me the desirability of investigating the effect of mere roughness of surface, all other circumstances being alike, and the disk of the radiometer being metallic, so that the two faces may be regarded as practically at the same temperature. Mr. Crookes's experiment, above referred to, led me to suspect that mere roughness might increase the efficiency of a surface; and I suggested to him some experiments with heated glass shades, or with a hot poker presented to the radiometer, the bulb being covered with a cool tumbler to defend it from being heated by the rays easily absorbed by glass. The result in every case answered my expectation; and it may be stated shortly that the law of the motion is that when the fly is hotter than the bulb the rough surface is repelled, or, say, the motion is positive; when cooler, negative.

I subjoin Mr. Crookes's memorandum of the results of experiment:—

"Aluminium Radiometer (1326), one side of the vanes being ruled closely with a sharp knife.

"1. Exposed to standard candle 3 inches off. Continuous *positive* rotation (ruled side repelled) at rate of 3¼ revolutions a minute.

"2. Exposed to non-luminous flame of a Bunsen burner 3 inches off. Continuous *positive* rotation at the rate of 7½ turns a minute.

"3. The Bunsen burner removed. The positive rotation gradually diminished till it stopped. No negative rotation.

"4. The bulb heated with Bunsen burner. Good *negative* rotation; then stopped, and rotated *positively* till quite cold.

"5. Bulb covered with a cold glass shade, and a large red-hot ring applied round equatorially. *Positive* rotation, but not very strong.

"6. On removing the shade and ring the positive movement soon comes to rest.

"7. Covered with a hot glass shade, *negative* rotation, with *positive* rotation on cooling (the same as 4).

"8. Plunged into hot water. *Negative* rotation.

"9. Removed from the hot water, and immediately plunged into cold. *Positive* rotation."

Results nearly identical were obtained with another radiometer described as "silver radiometer (1327), one side coated with finely divided silver, electro-deposited."

We must accordingly recognize three distinct conditions under which motion may be obtained in a radiometer, namely:—(1) difference of temperature of the two faces, as in a pith radiometer coated on one face with lampblack; (2) more favourable presentation of one face than the other, as in a radiometer with curved disks; (3) roughness of surface on one face (if this be really different from 2). These three conditions may be variously combined so as to assist or oppose each other, as the case may be, in producing motion.

December 20, 1877.

The Society then adjourned over the Christmas Recess, to Thursday, January 10, 1878.

Presents received, December 6, 1877.

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INDEX TO VOL. XXVI.

ABEL (F. A.), royal medal awarded to, 447.
Air, the examination of the (R. A. Smith), 512.
Airy (Sir G. B.) on the tides at Malta, 485.
Alps, observations on hermetically-sealed flasks opened on the (Tyndall), 487.
Ani (sphincter), automatic action of the (Gowers), 77.
Aniline, transformation of leucoline into (Dewar), 65.
Anniversary Meeting, Nov. 30, 1877, 426.
Annual Meeting for election of Fellows, June 7, 1877, 288.
Arctic seas, on the tides of the: Part VII. Tides of Port Kennedy, in Bellot Strait (Haughton), 63.
Asia and Europe, on the probable age of the continent of (Haughton), 401.
Atlantic cable, on the rapidity of growth and variability of some Madreporaria on an (Duncan), 133.
Attraction and repulsion of bubbles by heat (Hartley), 137.
Auditors, election of, 410; report of, 426.
Automatic action of the sphincter ani (Gowers), 77.

Bacteria, remarks on the attributes of the germinal particles of (Burdon Sanderson), 416.
——, researches on the effect of light upon, and other organisms (Downes and Blunt), 488.
Bakerian Lecture, on the organization of the fossil plants of the coal-measures (Williamson), 411.
Balance-sheet, 452.
Barkly (Sir Henry), admitted, 288.
Beds of chert in the upper carboniferous limestones of Ireland, on the nature and origin of the (Hull), 163.
Berthelot (M.), elected foreign member, 504.
Beryls, researches on emeralds and: Part II. (Williams), 165.
Bischof (G.) on putrescent organic matter in potable water, 152.
Blood, the physiology of sugar in relation to the (Pavy), 314; No. 2, 346.
Blunt (T. P.) and A. Downes, researches on the effect of light upon Bacteria and other organisms, 488.
Bosanquet (R. H. M.) on the Hindoo division of the octave, with some additions to the theory of systems of the higher orders, 372.
Brain, the relationship of the nerve-cells of the cortex to the lymphatic system of the (Lewis), 326.
Brazil, Emperor of, admitted, 288.
Brown (F. D.), the physical properties of homologues and isomers, 238.
Bubbles, on attraction and repulsion of, by heat (Hartley), 137.
——, on the constant vibration of minute (Hartley), 150.
Bunsen (R. W.), Davy medal awarded to, 450.

Candidates for election, list of, March 1, 1877, 1.
Carboniferous limestones of Ireland, on the nature and origin of the beds of chert in the upper (Hull), 163.
Chert, on the nature and origin of the beds of, in the upper carboniferous limestones of Ireland (Hull), 163.
Chick, note on the early stages of development of the nerves in the (Marshall), 47.
Chinoline series, studies in the: I. Transformation of leucoline into aniline (Dewar), 65.
Chloride-of-silver battery, experimental researches on the electric discharge with the (De La Rue and Müller), 519.
——, on the length of the spark between

two spherical surfaces of the (De La Rue and Müller), 324.
Christie (W. H. M.) on the magnifying-power of the half-prism as a means of obtaining great dispersion, and on the general theory of the half-prism spectroscope, 8.
Clarke (G. S.) and H. M'Leod on some figures exhibiting the motion of vibrating bodies, and on a new method for determining the speed of machines, 157.
Clifton (R. B.) on the difference of potential produced by the contact of different substances, 299.
Coal-measures, on the organization of the fossil plants of the: Part IX. Latest researches on the plants of the British coal-measures, especially of the *Calamites* and *Lepidodendra* (Williamson), 411.
Coleridge (Lord), elected, 210; admitted, 325.
Conduction (electrolytic) of some organic bodies (Gladstone and Tribe), 2.
Contact of different surfaces, on the difference of potential produced by the (Clifton), 299.
Copley medal awarded to J. D. Dana, 446.
Cortex, the relationships of the nerve-cells of the, to the lymphatic system of the brain (Lewis), 326.
Council, list of, 416, 451.
Creighton (C.), points of resemblance between the suprarenal bodies of the horse and dog, and certain occasional structures in the ovary, 500.
Crookes (W.) on repulsion resulting from radiation: preliminary note on the otheoscope, 176.
Crystalline plate, on the foci of lines seen through a (Stokes), 386.
Crystals, on some hitherto undescribed optical properties of doubly refracting (Sorby), 384.
Curves, on hyperjacobian surfaces and (Spottiswoode), 226.

Daily range of the magnetic declination, on the variations of the, as recorded at the Kew observatory (Stewart), 102.
Dana (J. D.), Copley medal awarded to, 446.
Darwin (F.) on the protrusion of protoplasmic filaments from the glandular hairs of the common teasel (*Dipsacus sylvestris*), 4.
Davy medal awarded to R. W. Bunsen and G. R. Kirchhoff, 450.
Decaisne (J.) elected foreign member, 504.
Declination (magnetic), on the variations of the daily range of the, as recorded at the Kew observatory (Stewart), 102.
Definite integrals, on certain (Russell), 359.
De La Rue (W.) and H. W. Müller on the length of the spark from a voltaic battery in different gases at ordinary atmospheric pressures, 227.
—— —— on the length of the spark between two spherical surfaces of the chloride-of-silver battery, 324.
—— —— experimental researches on the electric discharge with the chloride-of-silver battery, 519.
Density of solid mercury (Mallet), 71.
Dentine, on the structure and development of vascular (Tomes), 43.
Dewar (J.), admitted, 288.
——, studies in the chinoline series: I. Transformation of leucoline into aniline, 65.
Dipsacus sylvestris, 4.
Discharges, on stratified: IV. Stratified and unstratified forms of the jar-discharge (Spottiswoode), 90.
—— (——), photographic image of (Spottiswoode), 323.
Dispersion, on the magnifying-power of the half-prism as a means of obtaining great (Christie), 8,
Dog, suprarenal bodies of the horse and (Creighton), 500.
Donation Fund, account of grants from the, 1876–77, 457.
Downes (A.) and T.P. Blunt, researches on the effect of light upon Bacteria and other organisms, 488.
Du Bois-Reymond (E.) elected foreign member, 504.
Duncan (P. M.) on the rapidity of growth and variability of some Madreporaria on an Atlantic cable, with remarks upon the rate of accumulation of Foraminiferal deposits, 133.

Election of Fellows, 288.
Electric current, on the increase in resistance to the passage of an, produced on wires by stretching (Tomlinson), 401.
—— discharge with the chloride-of-silver battery, experimental researches on the (De La Rue and Müller), 519.
Electrolytes, distribution of the radicals of, upon an insulated metallic conductor (Tribe), 222.
——, production of induced currents in (Fleming), 40.
Electrolytic conduction of some organic bodies, note on the (Gladstone and Tribe), 2.
Electromotive properties of muscle, correction of statement in the note on the (Sanderson), 322.

Electrostatic capacity of glass (Hopkinson), 298.
Electrostriction, on (Mills), 504.
Elie de Beaumont (J. B. A. L. L.), obituary notice of, iv.
Emeralds and beryls, researches on: Part II. On some of the processes employed in the analysis of emeralds and beryls (Williams), 165.
Epidermis and subepidermic lymphatics, on the ultimate distribution of nerves to the (Hoggan), 289.
Evans (John), presentation of a portrait of Sir John Herschel, 410.
Ewing (J. A.) and F. Jenkin on friction between surfaces moving at slow speeds, 93.
Excitability of motor nerves, further observations on the modification of the (Romanes), 211.

Fayrer (Sir Joseph), admitted, 288.
Fellows elected, 288, 427; deceased, 427; number of, 451.
Ferments and germs, note on Dr. Burdon Sanderson's latest views of (Tyndall), 353.
Ferrers (Rev. N. M.), admitted, 288.
Filaments, on the protrusion of protoplasmic, from the glandular hairs of the common teasel (Darwin), 4.
Fleming (J. A.) on magneto-electric induction in liquids and gases: Part I. Production of induced currents in electrolytes, 40.
Foci of lines, on the, seen through a crystalline plate (Stokes), 386.
Foraminiferal deposits, remarks upon the rate of accumulation of (Duncan), 133.
Foreign members, election of, 504.
Fossil plants of the coal-measures, on the organization of the: Part IX. (Williamson), 411.
Fraser (Dr. T. R.), admitted, 325.
Frere (Sir H. Bartle E.), elected, 210.
Friction (on) between surfaces moving at low speeds (Jenkin and Ewing), 93.

Galton (F.), description of the process of verifying thermometers at the Kew observatory, 84.
Gases, on magneto-electric induction in liquids and: Part I. (Fleming), 40.
——, on the length of the spark from a voltaic battery in different, at ordinary atmospheric pressures (De La Rue and Müller), 227.
Geology (physical), notes on (Haughton), Nos. I. & II., 51; No. III. On the probable age of the continents of Asia and Europe, and on the absolute measure of geological time, 401; No. III. On a new method of finding limits to the duration of certain geological periods, 534.
Germinal particles of Bacteria, remarks on the attributes of the, in reply to Dr. Tyndall (Burdon Sanderson), 416.
Germs, note on Dr. Burdon Sanderson's latest views of ferments and (Tyndall), 353.
Gladstone (J. H.) and A. Tribe, note on the electrolytic conduction of some organic bodies, 2.
Glandular hairs of the common teasel, on the protrusion of protoplasmic filaments from the (Darwin), 4.
Glass, refractive indices of (Hopkinson), 290.
——, electrostatic capacity of (Hopkinson), 298.
Government fund of £4000, account of appropriations from the, 1877, 458.
Government grant of £1000, account of the appropriation of the, 1877, 457.
—— —— balance-sheet, 458.
Gowers (W. R.), the automatic action of the sphincter ani, 77.

Half-prism, on the magnifying-power of the, as a means of obtaining great dispersion, and on the general theory of the "half-prism spectroscope" (Christie), 8.
Haller (A. von), portrait of, presented by Dr. Sharpey, 411.
Hardman (E. T.), chemical notes on Mr. Hull's paper on the beds of chert, 163.
Hartley (W. N.) on attraction and repulsion of bubbles by heat, 137.
—— on the constant vibration of minute bubbles, 150.
Haughton (Rev. S.), notes on physical geology: Nos. I. & II., 51; No. III. On the probable age of the continent of Asia and Europe, and on the absolute measure of geological time, 401; No. III. On a new method of finding limits to the duration of certain geological periods, 534.
—— on the tides of the arctic seas: Part VII. Tides of Port Kennedy, in Bellot Strait (final discussion), 63.
Heat, on attraction and repulsion of bubbles by (Hartley), 137.
Heating and cooling, on certain molecular changes which occur in iron and steel during the separate acts of, 127.
Heer (Oswald), royal medal awarded to, 449.
Hermetically-sealed flasks, observations on, opened on the Alps (Tyndall), 487.
Herschel (Sir John), portrait of, presented by J. Evans, 410.
Hindoo division of the octave (Bosanquet), 372.
Hodgson (B. H.), admitted, 416.
Hoggan (G. and F. E.) on the minute structure and relationships of the lymphatics of the mammalian skin, and on

the ultimate distribution of nerves to the epidermis and subepidermic lymphatics, 289.
Homologues and isomers, the physical properties of (Brown), 238.
Hooker (Sir J. D.), President's address, 427.
Hopkinson (J.), refractive indices of glass, 290.
——, electrostatic capacity of glass, 298.
Horse and dog, suprarenal bodies of the (Creighton), 500.
Huggins (W.) on the inferences to be drawn from the appearance of bright lines in the spectra of irresolvable nebulæ, 179.
Hull (E.) on the nature and origin of the beds of chert in the upper carboniferous limestones of Ireland, 163.
Human body, on the temperature of the, in health (Ringer and Stuart), 186.
Hyperjacobian surfaces and curves (Spottiswoode), 226.

Induction, on magneto-electric, in liquids and gases: Part I. (Fleming), 40.
Induction-coefficients of magnets, on the temperature-correction and (Whipple), 218.
Integrals, on certain definite (Russell), 359.
Iron and steel, on certain molecular changes which occur in, during the separate acts of heating and cooling (Norris), 127.
Isomers, the physical properties of homologues and (Brown), 238.

Janssen (J.), photograph of the sun on glass presented by, 504.
Jar-discharge, stratified and unstratified forms of the (Spottiswoode), 90.
Jenkin (F.) and J. A. Ewing on friction between surfaces moving at low speeds, 93.
Judd (J. W.), admitted, 288.

Kerguelen, magnetic observations at (Perry), 95.
Kew committee, report of the, 1877, 461.
Kew observatory, description of the process of verifying thermometers at the (Galton), 84.
—— ——, magnetic observations made at the, 1876–77, 470.
—— ——, meteorological observations made at the, 1876-77, 476.
—— ——, on the variations of the daily range of the magnetic declination as recorded at the (Stewart), 102.
Kirchhoff (G. R.), Davy medal awarded to, 450.
Kolbe (A. W. H.) elected foreign member, 504.

Leuckart (R.) elected foreign member, 504.
Leucoline, transformation of, into aniline (Dewar), 65.
Lewis (B.), the relationships of the nerve-cells of the cortex to the lymphatic system of the brain, 326.
Life composition of £40, repeal of the statute relating to the, 325.
Light, researches on the effect of, upon Bacteria and other organisms (Downes and Blunt), 488.
Limestones (upper carboniferous) of Ireland, on the nature and origin of the beds of chert in the (Hull), 163.
Lines in the spectra of irresolvable star-clusters, on *a* cause for the appearance of bright (Stone), 156, 517.
—— in the spectra of irresolvable nebulæ, on the inferences to be drawn from the appearance of bright (Huggins), 179.
Liquids and gases, on magneto-electric induction in (Fleming): Part I., 40.
Lymphatic system of the brain, the relationships of the nerve-cells of the cortex to the (Lewis), 326.
Lymphatics of the mammalian skin, on the minute structure and relationships of the (Hoggan), 289.

Machines, a new method for determining the speed of (M'Leod and Clarke), 157.
M'Lachlan (R.), admitted, 288.
M'Leod (H.) and G. S. Clarke on some figures exhibiting the motion of vibrating bodies, and on a new method for determining the speed of machines, 157.
Madras, on the alleged correspondence of the rainfall at, with the sun-spot period (Strachey), 249.
Madreporaria, on the rapidity of growth and variability of some, on an Atlantic cable (Duncan), 133.
Magnetic declination, on the variations of the daily range of the, as recorded at the Kew observatory (Stewart), 102.
Magnetic observations at Kerguelen (Perry), 95.
—— —— made at the Kew observatory, 1876–77, 470.
Magneto-electric induction in liquids and gases (Fleming): Part I., 40.
Magnets, on the temperature-correction and induction-coefficients of (Whipple), 218.
Magnifying-power (on the) of the half-prism as a means of obtaining great dispersion (Christie), 8.
Mallet (J. W.) on the density of solid mercury, 71.
Malta, on the tides at (Airy), 485.
Marshall (A. M.), note on the early stages

of development of the nerves in the chick, 47.
Maskelyne (N. S.) and W. J. Russell, an attempt to form double salts of nitrate of silver and other nitrates, 357.
Medals, presentation of the, 446.
Mercury, on the density of solid (Mallet), 71.
Meteorological observations made at the Kew observatory, 1876–77, 476.
Mills (E. J.) on electrostriction, 504.
Molecular changes, on certain, which occur in iron and steel during the separate acts of heating and cooling (Norris), 127.
Moseley (H. N.), admitted, 410.
Motion of vibrating bodies, on some figures exhibiting the (M'Leod and Clarke), 157.
Motor nerves, further observations on the modification of the excitability of, produced by injury (Romanes), 211.
Müller (H. W.) and W. De La Rue on the length of the spark from a voltaic battery in different gases at ordinary atmospheric pressures, 227.
—— —— on the length of the spark between two spherical surfaces of the chloride-of-silver battery, 324.
—— ——, experimental researches on the electric discharge with the chloride-of-silver battery, 519.
Muscle, correction of statement in the note on the electromotive properties of (Sanderson), 322.

Nebulæ, on the inferences to be drawn from the appearance of bright lines in the spectra of irresolvable (Huggins), 179.
—— ——, *see* Stone.
Nerve-cells of the cortex, the relationships of the, to the lymphatic system of the brain (Lewis), 326.
Nerves, further observations on the modification of the excitability of motor, produced by injury (Romanes), 211.
—— in the chick, note on the early stages of development of the (Marshall), 47.
——, on the ultimate distribution of, to the epidermis and subepidermic lymphatics (Hoggan), 289.
Newcomb (S.) elected foreign member, 504.
Nitrate of silver, an attempt to form double salts of, and other nitrates (Russell and Maskelyne), 357.
Niven (W. D.) on the calculation of the trajectories of shot, 268.
Norris (R.) on certain molecular changes which occur in iron and steel during the separate acts of heating and cooling, 127.

Obituary notices of Fellows deceased:—
Dr. Sibson, i.
J. B. A. L. L. Elie de Beaumont, iv.
Octave, on the Hindoo division of the (Bosanquet), 372.
Optical properties of doubly refracting crystals, on some hitherto undescribed (Sorby), 384.
Organic bodies, note on the electrolytic conduction of some (Gladstone and Tribe), 2.
Organic matter, on putrescent, in potable water (Bischof), 152.
Organisms, further researches on the deportment and vital resistance of putrefactive and infective (Tyndall), 228.
Otheoscope, preliminary note on the (Crookes), 176.
Ovary, points of resemblance between the suprarenal bodies of the horse and dog, and certain occasional structures in the (Creighton), 500.

Paraffins, on the normal (Schorlemmer): Part II., 325.
Pavy (F. W.), the physiology of sugar in relation to the blood, 314; No. 2, 346.
Pedro II., Emp. of Brazil, admitted, 288.
Periodicity, on the true criterion of, in a series of variable quantities (Strachey), 249.
Perry (Rev. S. J.), magnetic observations at Kerguelen, 95.
Photographic image of stratified discharges (Spottiswoode), 323.
Physical geology, notes on (Haughton), Nos. I. & II., 51; No. III., 401, 534.
Physical properties of homologues and isomers (Brown), 238.
Physiology of sugar in relation to the blood (Pavy), 314; No. 2, 346.
Plants (fossil) of the coal-measures, on the organization of the: Part IX. (Williamson), 411.
Port Kennedy in Bellot Strait, tides of, final discussion (Haughton), 63.
Potable water, on putrescent organic matter in (Bischof), 152.
Potential, on the difference of, produced by the contact of different substances (Clifton), 299.
Presents, list of, 121, 181, 262, 363, 479, 555.
President's address, 427.
Prism (half-), on the magnifying-power of the, as a means of obtaining great dispersion (Christie), 8.
Protoplasmic filaments, on the protrusion of, from the glandular hairs of the common teasel (Darwin), 4.
Putrefactive and infective organisms, further researches on the deportment and vital resistance of (Tyndall), 228.
Putrescent organic matter in potable water (Bischof), 152.

Radiation, repulsion resulting from (Crookes), 176.

Radicals of electrolytes, distribution of the, upon an insulated metallic conductor (Tribe), 222.

Radiometers, on certain movements of (Stokes), 546.

Rainfall at Madras, on the alleged correspondence of the, with the sun-spot period (Strachey), 249.

Rayleigh (Lord) on the amplitude of sound-waves, 248.

Refracting (doubly) crystals, on some hitherto undescribed optical properties of (Sorby), 384.

Refractive indices of glass (Hopkinson), 290.

Reinold (A. W.) and A. W. Rücker on the thickness of soap films, 334.

Repulsion, on attraction and, of bubbles by heat (Hartley), 137.

—— resulting from radiation: preliminary note on the otheoscope (Crookes), 176.

Reynolds (Osborne), admitted, 288.

Ringer (S.) and A. P. Stuart on the temperature of the human body in health, 186.

Rivers, experimental demonstration in respect to the origin of windings of, in alluvial plains (Thomson), 356.

Roberts (W.), admitted, 288.

Romanes (G. J.), further observations on the modifications of the excitability of motor nerves produced by injury, 211.

Royal medal awarded to F. A. Abel, 447; to Oswald Heer, 449.

Rücker (A. W.) and A. W. Reinold on the thickness of soap films, 334.

Russell (W. H. L.) on certain definite integrals, 359.

Russell (W. J.) and N. S. Maskelyne, an attempt to form double salts of nitrate of silver and other nitrates, 357.

Saline solutions, on supersaturated (Tomlinson), 523.

Salts of nitrate of silver and other nitrates, an attempt to form double (Russell and Maskelyne), 357.

Sanderson (Burdon), correction of statement in the "note on the electromotive properties of muscle," 322.

——, remarks on the attributes of the germinal particles of Bacteria, in reply to Dr. Tyndall, 416.

——, his latest views of ferments and germs, note on (Tyndall), 353.

Schorlemmer (C.) on the normal paraffins: Part II., 325.

Sharpey (Dr.), letter relative to a portrait of Haller, 411.

Shot, on the calculation of the trajectories of (Niven), 268.

Sibson (Dr.), obituary notice of, i.

Silver (nitrate of), an attempt to form double salts of, and other nitrates (Russell and Maskelyne), 357.

Skin, on the minute structure and relationships of the lymphatics of the mammalian, 289.

Smith (R. Angus), the examination of the air, 512.

Soap films, on the thickness of (Reinold and Rücker), 334.

Sorby (H. C.) on some hitherto undescribed optical properties of doubly refracting crystals: preliminary notice, 384.

Sound-waves, on the amplitude of (Rayleigh), 248.

Spark, on the length of the, between two spherical surfaces of the chloride-of-silver battery, 324.

——, on the length of the, from a voltaic battery in different gases at ordinary atmospheric pressures (De La Rue and Müller), 227.

Spectra of irresolvable nebulæ, on the inferences to be drawn from the appearance of bright lines in the (Huggins), 179.

—— of irresolvable star-clusters, on *a* cause for the appearance of bright lines in the (Stone), 156, 517.

Spectroscope (half-prism), on the general theory of the (Christie), 8.

Speed of machines, a new method for determining the (M'Leod and Clarke), 157.

Speeds (low), on friction between surfaces moving at (Jenkin and Ewing), 93.

Sphincter ani, the automatic action of the (Gowers), 77.

Spottiswoode (W.) on hyperjacobian surfaces and curves, 226.

—— on stratified discharges: IV. Stratified and unstratified forms of the jar-discharge, 90.

——, photographic image of stratified discharges, 323.

Star-clusters, on *a* cause for the appearance of bright lines in the spectra of irresolvable (Stone), 156, 517.

—— ——, *see* Huggins.

Steel, on certain molecular changes which occur in iron and, during the separate acts of heating and cooling (Norris), 127.

Stewart (B.) on the variations of the daily range of the magnetic declination as recorded at the Kew observatory, 102.

Stokes (G. G.) on certain movements of radiometers, 546.

—— on the foci of lines seen through a crystalline plate, 386.

Stone (E. J.) on *a* cause for the appearance of bright lines in spectra of irresolvable star-clusters, 156, 517.

Strachey (General) on the alleged correspondence of the rainfall at Madras with the sun-spot period, and on the true criterion of periodicity in a series of variable quantities, 249.
Stratified discharges, on: IV. Stratified and unstratified forms of the jar-discharge (Spottiswoode), 90.
—— ——, photographic image of (Spottiswoode), 323.
Stretching, on the increase of resistance to the passage of an electric current, produced on wires by (Tomlinson), 401.
Stuart (A. P.) and S. Ringer on the temperature of the human body in health, 186.
Sugar, the physiology of, in relation to the blood, 314; No. 2, 346.
Sun-spot period, on the alleged correspondence of the rainfall at Madras with the (Strachey), 249.
Supersaturated saline solutions, on (Tomlinson), 523.
Suprarenal bodies of the horse and dog, points of resemblance between the, and certain occasional structures in the ovary (Creighton), 500.
Surfaces moving at low speeds, on friction between (Jenkin and Ewing), 93.
Surfaces, on hyperjacobian, and curves (Spottiswoode), 226.

Teasel, on the protrusion of protoplasmic filaments from the glandular hairs of the common (Darwin), 4.
Temperature of the human body in health (Ringer and Stuart), 186.
Temperature-correction and induction-coefficients of magnets (Whipple), 218.
Thermometers, description of the process of verifying, at the Kew observatory (Galton), 84.
Thomson (J.), admitted, 288.
——, experimental demonstration in respect to the origin of windings of rivers in alluvial plains, and to the mode of flow of water round bends of pipes, 356.
Tides at Malta (Airy), 485.
—— of the arctic seas: Part VII. Tides of Port Kennedy, in Bellot Strait: final discussion (Haughton), 63.
Time, on the absolute measure of geological (Haughton), 401.
Tomes (C. S.) on the structure and development of vascular dentine, 43.
Tomlinson (C.) on supersaturated saline solutions, 523.
Tomlinson (H.) on the increase in resistance to the passage of an electric current produced on wires by stretching, 401.
Trajectories of shot, on the calculation of the (Niven), 268.
Tribe (A.), distribution of the radicals of electrolytes upon an insulated metallic conductor, 222.
—— and J. H. Gladstone, note on the electrolytic conduction of some organic bodies, 2.
Trust funds, 454–456.
Tschebytschew (P.) elected foreign member, 504.
Turner (W.), admitted, 288.
Tyndall (J.), further researches on the deportment and vital resistance of putrefactive and infective organisms, from a physical point of view, 228.
——, note on Dr. Burdon Sanderson's latest views of ferments and germs, 353.
—— ——, reply to, by Dr. Burdon Sanderson, 416.
——, observations on hermetically-sealed flasks opened on the Alps, 487.

Vascular dentine, on the structure and development of (Tomes), 43.
Vibrating bodies, on some figures exhibiting the motion of (M'Leod and Clarke), 157.
Vibration, on the constant, of minute bubbles (Hartley), 150.
Vice-Presidents appointed, 485.
Voltaic battery, on the length of the spark from a, in different gases at ordinary atmospheric pressures (De La Rue and Müller), 227.

Water, on putrescent organic matter in potable, 152.
——, on the mode of flow of, round bends of pipes (Thomson), 356.
Whipple (G. M.) on the temperature-correction and induction-coefficients of magnets, 218.
Williams (G.), researches on emeralds and beryls: Part II. On some of the processes employed in the analysis of emeralds and beryls, 165.
Williamson (W. C.) on the organization of the fossil plants of the coal-measures: Part IX. on the latest researches into the organization of the fossil plants of the British coal-measures, especially of the *Calamites* and *Lepidodendra*, 411.
Windings of rivers in alluvial plains, experimental demonstration in respect to the origin of (Thomson), 356.

END OF THE TWENTY-SIXTH VOLUME.

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OBITUARY NOTICES OF FELLOWS DECEASED.

Dr. **Sibson**, born in 1815 at Cross Canonby, near Maryport, in Cumberland, was educated in Edinburgh, where he also commenced the study of medicine. He was a pupil of Lizars, and in his pupilage, in 1831–2, volunteered to serve in the Cholera Hospital, and in that first and terrible epidemic risked his life. Then he entered at Guy's; and many who remember him there tell of the rare energy with which he gave himself to study both in the wards and, especially, in the pathological theatre. Among those whose admiration he attracted was the late Dr. Hodgkin, who, from that time, never ceased to encourage him in all his labour and to aid him in every way that could lead to success.

In 1835 he was appointed resident surgeon to the Nottingham General Hospital, and during that appointment, which he held till 1848, gained a considerable knowledge not only of medicine, but of surgery, of which, in later life, he often made good use in consultations. There, also, he made his chief investigations into the mechanical physiology of respiration and the position of the internal organs in health and disease, which were throughout his life a principal subject of his studies; and through these his name will always be honourably remembered in the history of medicine. They brought him early into high repute when first published in 1844 in the 'Provincial Medical and Surgical Transactions,' and subsequently in a separate volume. His great work on Medical Anatomy, towards which most of his researches led, and in which all his chief results may be studied, was published in 1869. It is a book to be commended for its thoroughness. Its preparation involved severe labour, most of which was carried on in the dead-house of the Marylebone Infirmary.

In 1848, at the end of his tenure of office at Nottingham, Mr. Sibson graduated at the University of London, and passed in the same year the examinations for M.B. and M.D., gaining honours in both—a noteworthy achievement, considering the active life of practical work in which he had long been engaged. He remained always devoted to the University, fulfilled for some years the duties of examiner in medicine, and in 1865 was made a member of the Senate, on the nomination of Convocation. Few were so constant as he in attention to all the business of the university that had relation to the medical or other natural sciences. Alike in the senate, in committees, and in convocation, he was zealous for the promotion of all good designs.

The like may be said of his work in the College of Physicians, of which he became a member in 1849 and a Fellow in 1853. In this year he delivered the Gulstonian Lectures, in 1870 the Croonian, in 1873 the

Lumleian; but his chief work was in the office of secretary to the Committees on the Nomenclature of Diseases. It would be impossible to overestimate the value of his services in that capacity. He was the principal originator of the undertaking, and never ceased to labour in it during the six or more years in which it was in progress. Of all the great honour due to a work of so large importance, even of much more than national importance, the greater part was certainly gained by Dr. Sibson.

Dr. Sibson was elected one of the physicians of St. Mary's Hospital in 1851, and held the post during twenty years: he was also for some time one of the lecturers on medicine in the school. His teaching was earnest, laborious, and minute. Emphatic as he was in speech, and never for a moment doubting the importance of even the smallest fact, he made all listen to him, and he made many learn. Few, indeed, could follow him in all the minuteness of detail with which he spoke on his chief subjects, such as the diagnosis of diseases in the chest; but he made his pupils feel that they had work to do for which nothing less than the devotion of a life could suffice, and he showed them how to do it.

Dr. Sibson was not less vigorous in the British Medical Association. He first became a member in 1843; in 1850 he delivered the address in physiology, in 1870 that in medicine; for many years he was a member of the General Council, for three years its president, and after this a permanent vice-president. It was at his suggestion that the plan for grants for scientific researches was adopted, and he was chairman of the Grant-Committee. Briefly, he took part in every good work of the Association.

And he took a willing part in larger and more public works. He was a very active member of the Asylum District Board and of the Government Commission in the Greenwich Hospital Enquiry in 1867–8.

He was elected a Fellow of the Royal Society in 1849 after the publication of two papers in the 'Transactions,' became a member of the Council in 1872, and was for some years Treasurer of the Royal Society Club. Among his honours he had the honorary degree of M.D. of Dublin and of LL.D. of Durham*.

It is not possible to give a summary of Dr. Sibson's numerous published essays, comprising, in addition to the subject above mentioned, researches on narcotic poisons, ether, and chloroform. It was while investigating the action of anæsthetics that, by making use of soft lead as the most pliable substance for adaptation to the mouth, he succeeded in constructing a very ingenious mask to be worn during inhalation; and to him the profession is indebted for the double valve for expiration and inspiration.

Nearly every one of the essays relating to the chest was in itself a summary of minute and laborious studies. The characteristic of all this part of his work was in his devotion and immense labour to determine the questions which he set himself. Perhaps he overestimated the relative importance of some of them; but even if he did he should hardly be charged with fault; rather his example may be studied as a protest against the greater and much more common fault of thinking that facts are easy to be established. He will probably always enjoy the rare honour of having settled some things so certainly that they will never need to be investigated again.

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* Facts derived from an excellent memoir of Dr. Sibson in the 'British Medical *Journal*,' September 30, 1876.

It may be doubted whether we had in our Society a man of stronger will for work than Sibson. He never flinched from any duty; he never tried to make it easy: when he saw a duty to be done he looked to see how large it could be made, how manifold in detail; and he did it all. No man followed better the advice of the king Preacher, "Whatsoever thy hand findeth to do, do it with thy might." And surely no one ever worked harder with as light and genial a heart. Who of us can forget the gentleness and enthusiasm of his social life, his fervent greetings, his words of affection, the sincerity of which was proved by the whole tenour of his pure unselfish life? He was a many-sided man, and on all sides good; a true lover of nature and of art, his house was adorned with a fine collection of engravings, and especially Wedgwood ware, of which he was a critical judge: his collection of Wedgwood medallions of scientific men (unfortunately dispersed by sale after his death) was probably the most complete ever got together, and was especially rich in representations of Fellows of the Royal Society.

Dr. Sibson married, in July 1858, Sarah Mary, younger daughter of Peter Aimé Ouvry, Esq., of East Acton, a lady of highly cultivated mind and of rare artistic accomplishments. His death was, as he expected, sudden. He died at Geneva on the 7th of September, on his way home from a vacation tour; and when we might have thought of him as coming to us again, with his enthusiastic narratives of adventure or of some study in rare art, abrupt news came that his career was finished[*].

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[*] Dr. Sibson's chief papers, besides those already referred to, may be found in the 'Philosophical Transactions,' 1846, 1848, vols. 136, 138; the 'London Medical Gazette,' 2nd ser., vols. iv., vi., vii., viii., x.; 'London Journal of Medicine,' vol. i.; 'British Medical Journal,' 1873; 'Pathological Transactions,' vols. ii., iii., x.; 'Trans. of British Medical Association,' vol. xvii., 1850; Reynolds's 'System of Medicine,' art. "Pericarditis, Endocarditis" (these were completed after the labour, it is said, of three years, just before his death). The 'British Medical Journal' of the current year contains the Harveian Lectures on Bright's disease, which Dr. Sibson had prepared for delivery not long before his death.

J. B. A. L. L. **Élie de Beaumont** was born on the 25th September, 1798. In the year 1817 he became a student at the École Polytechnique, where he greatly distinguished himself. In 1819 he left that School, having the first place among those who were leaving at the same time, and commenced his studies at the École des Mines. He soon evinced a decided taste for geological pursuits, and in his second year his excellent and distinguished master, M. Brochant de Villiers, wrote of him as follows:—"Il est regardé comme un de nos plus forts sujets présents et passés. Il a surtout un grand goût et beaucoup de dispositions pour la Géologie." The papers he wrote after various geological excursions made by the pupils have been preserved in the School and show his zeal and capacity for that study.

In the beginning of his career as an engineer his masterly memoir on the sandstone strata of the Vosges attracted much notice, and at once gave him a place among rising geologists.

In 1822, just as the pupil engineer was completing his brilliant course of study, an incident decided his career. The Council of the École des Mines having received a copy of the fine Geological map of England, just executed, were so much struck with the importance of the work that they expressed to the Administration a wish that a similar map of France should be drawn up.

Before this suggestion could be carried out, it was necessary to gain a large mass of information from England; and Élie de Beaumont was soon afterwards sent with Brochant de Villiers, and his friend and colleague, Dufrénoy, on a mission to England, in order to study the principles on which this map had been prepared. On their return they published a description of the principal metallurgical establishments of this country, then little known to French manufacturers. The Geological conditions of the workings in Great Britain, the processes employed in manufactories, and the general arrangements of plant were carefully described in this elaborate work, which is considered to have rendered an immense service to French Metallurgy. As a consequence of this mission Élie de Beaumont and Dufrénoy were commissioned, under the direction of Brochant de Villiers, to prepare a Geological Map of France, a task which, begun in 1825, occupied eighteen years.

In 1824 Élie de Beaumont was appointed as Engineer of second class at Rouen, and held that position until 1827, when he was placed as assistant to M. Brochant de Villiers in the duties of the Geological Chair at the École des Mines, and in 1835 succeeded to the professorship.

Hitherto Élie de Beaumont had been chiefly remarkable for great power of work and intelligence. He was now to take high rank as an original investigator. In 1829 he made a communication to the Academy, in which he asserted that the oldest chain of mountains in France was that of the Côte d'or, in Burgundy, that the Pyrenees and Apennines were of later date, that Mont Blanc belonged to a still more recent period, and that the St. Gothard was younger than Mont Blanc. The most eminent geologists of the time adopted the new doctrines, which were enthusiastically propagated by Arago. Thus encouraged, Élie de Beaumont prosecuted his studies of the origin of mountain-ranges with renewed vigour, and his 'Systèmes des Montagnes' was the result, followed in after years by large additions of facts and the development of his great idea of the pentagonal network of mountain-chains. His views have not met with universal concurrence either from geologists or mathematicians. They have been ably controverted by other geologists, among whom are Sir Charles Lyell and Mr. Hopkins in his anniversary address to the Geological Society in 1853.

In 1832, on the death of Cuvier, Élie de Beaumont was elected almost unanimously to the vacant chair in the Collége de France. There he created a school of Geology, and for upwards of twenty years his lectures were numerously attended by students of geology.

He was early in his career elected into the French Academy, and on the death of Arago in 1853 he was selected for the important post of Perpetual Secretary of that learned body, which post he retained until his unexpected death on the 21st September, 1874.

After long service as Inspector-General of Mines he became, in 1861, Vice-President of the Conseil-Général des Mines, and a Grand Officer of the Legion of Honour. He was also a member of the French Senate. For nearly 50 years of his life (that is to say, from his entrance as pupil in 1819 until his superannuation in 1868) he was attached to the École des Mines.

The extent and number of his writings can only be judged of by a complete list; but his 'Géologie Pratique' may especially be noticed.

M. Élie de Beaumont married a lady of the distinguished house of Quélen. Her death and the miseries of the late war no doubt tended to hasten his end.

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